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CSICOP AND THE MARS EFFECT



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Zetetic scholar

CONTENTS

NUMBER 9

MARCH 1982

ARTICLES

JEFF SOBAL AND CHARLES F. EMMONS Patterns of Belief in Religious, Psychic and Other Paranormal Phenomena.....	7
PIET HEIN HOEBENS Mystery Men from Holland, II: The Strange Case of Gerard Croiset (A CSAR Special Projects Report).....	20
MICHAEL MARTIN Defining "UFO".....	84
MICHAEL HARRISON Reflections on the Role of Hyperosmia in ESP: Some Personal Observations (A Viewpoints Essay).....	98

NEW ZS DIALOGUE

Introduction to "Research on the Mars Effect" (M. TRUZZI).....	33
PATRICK CURRY Research on the Mars Effect.....	34
Critical Commentaries By: MICHEL GAUQUELIN..... 54 PIET HEIN HOEBENS..... 70 HANS J. EYSENCK..... 61 LUC DE MARRE..... 71 H. KRIPS..... 63 J. DOMMANGET..... 73 I.J. GOOD..... 65 GAUQUELIN replies..... 75	
PATRICK CURRY Replies to His Commentators.....	78

CONTINUING ZS DIALOGUES

Comments on J. Richard Greenwell re UFOs by: RICHARD DE MILLE.....	96
GEORGE O. ABELL.....	96
J. ALLEN HYNEK.....	97

REGULAR ZS FEATURES

EDITORIAL.....	3
LETTERS: MALCOLM DEAN * IVAN W. KELLY.....	90
RANDOM BIBLIOGRAPHY ON THE OCCULT AND THE PARANORMAL.....	68

BOOK REVIEWS

John Hasted's METAL BENDERS (Harry Collins).....	108
--	-----

Books Briefly Noted (M. TRUZZI).....	110
--------------------------------------	-----

ERRATA.....	20
-------------	----

ON CSAR.....	6
--------------	---

ABOUT THE CONTRIBUTORS TO THIS ISSUE.....	2
---	---

ANOMALY NEWSFRONT.....	97
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ABOUT THE CONTRIBUTORS TO THIS ISSUE:

GEORGE O. ABELL is a Professor of Astronomy at the University of California at Los Angeles.

HARRY COLLINS is a Lecturer in Sociology in the School of Humanities and Social Sciences at the University of Bath.

PATRICK CURRY is a philosopher of science with special interest in the history of astrology, and he now resides in London.

MALCOLM DEAN is a science writer and the author of The Astrology Game. He resides in Toronto.

LUC DE MARRE is an academic in Antwerp, Belgium, and a former member of the Comité Para.

RICHARD DE MILLE is a psychologist and author whose most recent book is The Don Juan Papers: Further Castaneda Controversies.

J. DOMMANGET is an astronomer and President of the Comité Belge pour l'Investigation Scientifique des Phénomènes réputés Paranormaux.

CHARLES F. EMMONS is an Associate Professor of Sociology at Gettysburg College and author of Chinese Ghosts and ESP: A Study of Paranormal Beliefs and Experiences.

HANS J. EYSENCK is a Professor of Psychology at the University of London and the co-author of Astrology--Science or Superstition? (in press).

MICHEL GAUQUELIN is a psychologist and the Director of the Laboratoire d'Etude des Relations entre Rythmes Cosmiques et Psychophysiologiques in Paris.

I.J. GOOD is the University Distinguished Professor of Statistics at Virginia Polytechnic Institute and State University.

MICHAEL HARRISON is a writer, residing in Hove, Sussex, England, and the author of many books including Fire From Heaven: A Study of Spontaneous Combustion.

PIET HEIN HOEBENS is a journalist and editorial writer for De Telegraaf in Amsterdam, Holland, and is a frequent writer on parapsychology.

J. ALLEN HYNEK is Professor Emeritus in Astronomy at Northwestern University and Director of the Center for UFO Studies.

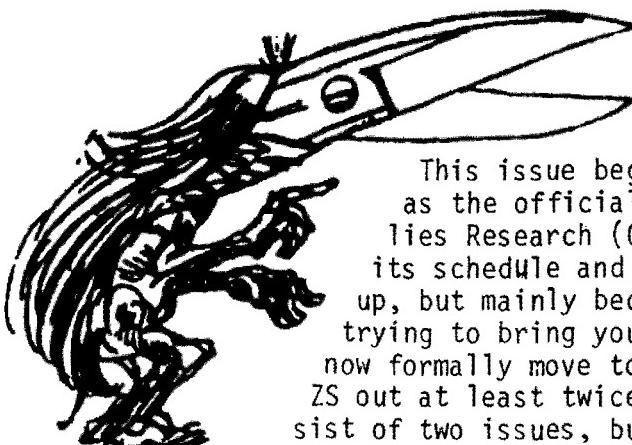
IVAN W. KELLY is an Assistant Professor of Educational Psychology at the University of Saskatchewan.

HENRY KRIPS is a Professor of History and Philosophy of Science at the University of Melbourne.

MICHAEL MARTIN is Professor of Philosophy at Boston University whose many works include his co-edited Probability, Confirmation, and Simplicity.

JEFF SOBAL is an Assistant Professor in the Department of Family Medicine at the University of Maryland School of Medicine,

MARCELLO TRUZZI is a Professor of Sociology and Sociology Department Head at Eastern Michigan University.



EDITORIAL

This issue begins the first publication of ZETETIC SCHOLAR as the official journal of the Center for Scientific Anomalies Research (CSAR). In part because ZS has gotten off of its schedule and there seems little point in trying to catch up, but mainly because the character of the dialogues we are trying to bring you make a rigid schedule difficult, ZS will now formally move to an irregular schedule. We will try to bring ZS out at least twice per year, and subscriptions will still consist of two issues, but issues will now simply be numbered and come out as content and conditions best warrant. Thus, there should be two issues coming out this year (1982), but there was only one issue (#8) published during 1981 (though #7, dated Dec. 1980, actually reached most readers in 1981. All this should be of little concern for our individual subscriber-readers, but it is important that we mention this for the benefit of our library subscribers who need to catalog our journal.

SOME REFLECTIONS ON THE CSICOP/MARS-EFFECT CONTROVERSY: A PERSONAL VIEW

This issue of ZS includes an important dialogue on the controversy around the Mars Effect experiments sponsored by the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). What follows reflects only the views of myself, and should not be taken as the viewpoint of any of ZS's Associate or Consulting Editors or as the view of any Senior Consultants to CSAR. Some of them might wish to express their disagreement in a future issue of ZS.

After I resigned from CSICOP over philosophical differences (I had been a founder, co-chairman, and editor for its journal, then THE ZETETIC), I found myself in an awkward position. I still hoped that CSICOP might be able to live up to its stated goals which included avoidance of pre-judgement and objective and impartial inquiry. I still consider myself a skeptic and respect many of the individuals connected with CSICOP, many of whose views really differ little from my own. Rather than publicly challenge CSICOP over my differences with its leadership, and perhaps create a schism (though some nonetheless accused me of that at the time I resigned), I chose what I thought a more constructive direction. I founded ZS (and now CSAR) to bring together those in CSICOP and those involved with scientific research programs advocating claims of the paranormal. This was to produce a "third force" which might complement rather than compete with either side. In short, I tried to bring together representatives ("lawyers") from all sides who shared a common concern with scientific rules of evidence and argument. For the most part, this has been successful, and today ZS and CSAR have received support from significant advocates from all sides.

Unfortunately, CSICOP leadership continues to publicize CSICOP as being more than mere advocates for the dominant scientific viewpoint which rejects claims of the paranormal. Much of CSICOP's publicity emphasizes its desire to protect the public from "irrationality" and "pseudoscience," and speaks of these as dark forces that could subvert the progress of civilization. Though I consider that position to be naive philosophically, sociologically, and historically, and one reflecting a zeal and prejudice incompatible with impartial and objective research, such advocacy can be an important

counter-balancing force for advocacy by proponents of the paranormal. We need good "lawyers" on all sides of the issues involved. But, alas, much of CSICOP's publicity goes well beyond advertising CSICOP as a group of responsible and skilled advocates against claims of the paranormal. That advertising presents CSICOP as an impartial and truth-seeking body which can screen claims and protect the rest of us from "nonsense." It presents CSICOP as arbiters for what is truly rational and scientific, as an objective body to act as gatekeeper for science as well as an educator for the general public. In short, CSICOP is portrayed as a scientific "judge" rather than as a mere "lawyer" or member of the "jury" that is constituted by the whole of the scientific community.

There are mixed feelings about these two roles (advocate vs. exemplar and gatekeeper) within CSICOP, even among its Councilors. Some have described CSICOP as a "lobby group" and simply as an important advocate for "the other side." Others, especially its chairman, have depicted CSICOP as carrying forward a Great Crusade against the Irrational and some sort of scientific savior of Civilization. If the former viewpoint had dominated CSICOP, and the leadership had been willing to make that clear in its publicity, I might never have felt compelled to resign from CSICOP; but I felt then--and more strongly now-- that the latter view of CSICOP was not only false but could even act as a barrier to inquiry.

By holding most pro-paranormal researchers in disdain and labelling them as pseudoscientists rather than as fellow scientists who might simply hold theories which might ultimately be discredited by evidence and argument, CSICOP put itself into an adversarial relationship with the proponents. (It also to a large degree prejudged as pseudoscientific the very claims which only inquiry could demonstrate really to be false science.) This adversarial relationship converted dialogue into debate where one side wins and the other loses. Worst of all, any validation of an anomaly claimed by proponents would be viewed as a "defeat" for CSICOP, even though any new discovery should be seen as a victory for science which can only aid its progress. While science should mainly seek to explain, the CSICOP became centrally concerned with "explaining away." For an individual scientist, there is nothing wrong with thinking that Gauquelin's Mars Effect or any other anomaly claim is probably wrong and setting about doing research seeking to demonstrate such error. But when that is the central orientation of CSICOP, it no longer can claim to place the goal of inquiry above the goal of advocacy. And that is the basic problem now. CSICOP has proclaimed its major aim to be inquiry while actually being centrally concerned with advocacy (i.e., discrediting claims of the paranormal). It is for this reason that many proponents of the paranormal feared that CSICOP might turn into a new kind of Inquisition (after all, even the Roman Catholic Inquisitors claimed that all they were really after was Truth through inquiry).

Until now, I have generally tried to ignore my differences with CSICOP in the pages of ZS. But the current dispute over the Mars Effect has brought the issue into sharp relief. If the charges made by ex-CSICOP Councilor Dennis Rawlins and Patrick Curry against CSICOP's handling of their tests is correct, which one of the two images we hold of CSICOP's role will influence how we assess the significance of any scientific errors they may have made. If CSICOP is merely seen as a bunch of advocates who take a particular scientific posture towards their research, they have merely conducted two controversial pieces of research, neither of which might have normally passed the referee standards for our best scientific journals.

Science is a self-correcting system, and poor science gets done frequently and gets regularly corrected. We should remember that even honest scientists can make mistakes, that scientists are all too human, and simply let it go at that. This is the position taken by some defenders of CSICOP, including some CSICOP Councilors, who see the whole controversy as a "tempest in a teapot." If CSICOP had been promoting itself as just a bunch of like-minded scientists-advocates, we could treat all this as minor, just as we might treat mistakes made in a parapsychological laboratory as having few serious implications beyond that particular laboratory.

But CSICOP has represented itself as far more than just a group of advocates. It has represented itself as a paragon for science. It supposedly includes the best and brightest and the most responsible elements of the scientific community. It has held itself up as a model for all of us interested in doing research on the paranormal. It advertises itself as a Guardian of Rationalism and as a Defender of True Science against the pseudoscientific hordes at our gates. Given that presentation of itself (one which some of us who consider ourselves rationalists resent), CSICOP should expect to be held to a higher standard of excellence than we might apply to more modest scientists. We should expect CSICOP, like Caesar's wife, to seek to be beyond reproach or scandal. Thus, if we accept CSICOP's own image of itself as a paragon, we must conclude that its errors are significant indeed. And those who have allowed CSICOP to present itself as a spokesman for Rationalism must surely recognize that these charges--if valid--would give a black eye to rationalism as well as to the individual scientists who may have conducted the imperfect work.

Alas, the significance of these alleged (and some now admitted) errors by CSICOP have been perceived as potentially tarnishing Rationalism among those who would still defend CSICOP as paragons. Stuck with the public image of exemplar that they put forward, the CSICOP leadership seems to have reacted to the charges in political rather than scientific fashion. Rather than simply answer the charges in detail, or simply admit to error fully and openly while taking action to restructure CSICOP so as to avoid future errors (and perhaps even acknowledge that the image as paragon was false and act to transform its public image into that of honest advocate), CSICOP seems to be standing aloof from the charges and avoiding detailed reply. This has led some to conclude that even if there was no original coverup as Rawlins has charged, the reaction (that is the non-reaction or stonewalling) to those charges amounts to a coverup going on now. In any case, silence can speak eloquently, and continued silence can not enhance CSICOP's credibility.

All of this is tragic, for in the long run, it will not encourage inquiry. Further, the CSICOP leadership has recently decided that CSICOP will no longer conduct or sponsor research, it will only publish that done by others. (It would also appear that CSICOP is closing down further discussion of the Mars Effect or the controversy surrounding it for the pages of its own journal, an action that may avoid some further controversy but hardly promotes the goal of continuous inquiry.) This will functionally diminish the original meaning of "Scientific Investigation" in the title of the Committee, originally one of CSICOP's major publicly stated objectives. The tragedy in all this is that we sorely need responsible advocates doing research to check out and counter the experiments being done by advocates of the paranormal. Science needs a group holding itself up as a paragon far less than it needs more and better research. The major business of science is not the upholding of authority; it is the advancement of research and explanation.

--MT

CENTER FOR SCIENTIFIC ANOMALIES RESEARCH

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* To advance the interdisciplinary scientific study of alleged and verified anomalies. * To act as a clearinghouse for scientific anomaly research. * To publish a journal (*Zetetic Scholar*), a newsletter (*The CSAR Bulletin*), research reports, and bibliographies. * To promote dissemination of information about scientific anomaly research. * To create a public network of experts on anomaly research through publication of a *CSAR Directory of Consultants*. * To sponsor conferences, lectures and symposia related to anomaly research. * To promote improved communication between critics and proponents of scientific anomaly research.

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ADDRESS: P.O. Box 1052

ASSOCIATE DIRECTOR OF CSAR: Ronald Westrum

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The primary focus of the Center will be on the study and evaluation of bodies of anomalous observations rather than upon esoteric theories seeking to explain already known phenomena. The orientation of the Center is exclusively scientific, places the burden of proof on the claimant, and recognizes the need for a degree of proof commensurate with the extraordinary character of the phenomenon being claimed. But the center also wishes to promote open and fair-minded inquiry that will be constructively skeptical. We recognize that scientific anomalies, where valid, may be instruments and driving forces for reconceptualization and growth in scientific theory. Critically and constructively approached, legitimate anomalies should be welcomed by science rather than perceived as ill-fitting nuisances. History clearly demonstrates that tomorrow's science is likely to contain surprises, and tomorrow's theories are likely to explain some of what are today viewed as controversial anomalies. Also, tomorrow's explanatory theories may be in areas of science not now perceived as relevant to the anomalies being considered. Thus, "anomalistics" must necessarily be an interdisciplinary endeavor.

Membership in CSAR is not yet opened but that will be announced in ZS#10. Still in its formative stage, CSAR is seeking applications for its *Directory of Consultants*. Being listed as a Research or Resource Consultant in the directory is merely intended to promote an international network of those with expertise on anomalies. Consultants to CSAR are not necessarily Members, and listing means neither their endorsement of CSAR nor CSAR's endorsement of the Consultants' views. The *Directory* is simply intended as a kind of "Who's Who" in anomalistics research and nothing more. We naturally hope that Consultants listed will want to become actively involved in the work of CSAR, but being designated a Research or Resource Consultant merely means that CSAR has approved your inclusion in its *Directory*.

PATTERNS OF BELIEF IN RELIGIOUS, PSYCHIC, AND OTHER PARANORMAL PHENOMENA *



JEFF SOBAL & CHARLES F. EMMONS

In our culture there are widespread beliefs in many phenomena not recognized or explained by current scientific paradigms. Some of these unexplained phenomena are based in religious tradition, such as life after death, angels, and devils. Other beliefs derive largely from secular popular culture, including ESP (extrasensory perception), clairvoyance, ghosts, and others. To be sure, especially some of the latter have been the subject of investigation on the part of parapsychologists and other scientists, but this paper will examine public belief in such occurrences without considering arguments about their validity. This approach is consistent with the classic dictum of W.I. Thomas (1928): "If men define situations as real, they are real in their consequences." Thus the importance for society of witches, Sasquatch, or astrology is not necessarily whether they exist as operable identities, but the fact that at least some people change their own lives and the lives of others by believing that and acting as if they did exist.

However, parapsychology itself may also benefit from a consideration of patterns of belief in paranormal phenomena. Although parapsychologists have devoted considerable attention to the effects of personality and of other psychological and situational factors on PSI performance, there has been relatively little emphasis on social or social-psychological factors. One outstanding exception has been the research on the "sheep-goat effect" (e.g., Palmer, 1971). A more sophisticated analysis of "sheep" (believers) could be integrated with future laboratory experimentation.

Conscious of a revival of interest in the paranormal in the late 1960's (Heenan, 1973), sociologists have made some efforts to apply perspectives especially from the sociology of religion (Greeley, 1975) and of popular culture (Truzzi, 1972), but also of the sociology of knowledge and social change (Tiryakian, 1972) to belief and involvement in unexplained phenomena. However, there has been little delineation of the broad spectrum of beliefs, with much of the attention of the sociology of the paranormal focused only on astrology (Wuthnow, 1976; Truzzi, 1975).

A major question which must be confronted is how to differentiate types of belief and types of believer. Religious institutions provide frameworks for interpreting the world. Modern science also generates paradigms that become incorporated into popular worldviews. Yet neither, nor both, can explain everything, nor create airtight "plausibility structures" (Berger, 1969) for all. Are beliefs in nonreligious unexplained phenomena (e.g., ESP) really nonscientific (or antiscientific) and therefore highly correlated

* The authors would like to thank Karl Beverly for assistance in data processing, the Dept. of Sociology and Anthropology at Gettysburg College for its support, and the American Institute of Public Opinion for making data available for secondary analysis.

with beliefs in religious paranormal phenomena (e.g., life after death), or do they represent an underlying factor or factors separate both from science and from religion? Data will be presented here both on the prevalence and on the patterns of such beliefs.

The Sample

Information about belief in unexplained phenomena was gathered in a survey by the American Institute of Public Opinion (Gallup) on February 24 to 27 of 1978. A total of 1553 adults (18 years or older) were administered standardized personal interviews in their homes. The sampling universe was the noninstitutionalized population of the United States, which was stratified geographically into seven regions (New England, Middle Atlantic, East Central, West Central, South, Mountain, and Pacific) and stratified by population into four categories of city size based upon 1970 census data (over 1 million, 1 million to 250,000, 249,999 to 50,000, under 50,000). This stratification was used to produce 362 sampling locations, where an average of 4.29 respondents were interviewed at each area in what was essentially a single stage systematic sample. Interviewers at each location collected the one call sample by beginning with a random start and then contacting each household on a map in sequence, continuing until their assigned number of interviews was completed.

Concepts and Indicators

The concept of belief in unexplained or paranormal phenomena could be measured by asking about a wide variety of topics. Here respondents were asked "Which of the following do you believe in:" and presented with a card listing: Ghost, the Loch Ness Monster, Sasquatch, ESP, Witches, Deja Vu, Precognition, Astrology, Angels, Devils, Life after Death, Clairvoyance. A yes or no response was recorded for each. Demographics were measured by standardized, pretested direct questions about age, marital status and education (last grade completed in school). Race and sex were coded by the trained, professional interviewers without asking the respondent.

Results

The percentage of people reporting that they believe in each of the dozen paranormal phenomena, along with the belief score and standard deviation of each phenomena, is shown in table 1.* There is considerable variation in the percentage of believers in each phenomenon, suggesting no simple pattern of belief. In general, there was a high level of belief in the religious phenomena of life after death (63%), angels (54%), and devils (39%), along with ESP (50%). A lower level of belief existed for precognition (37%), astrology (29%), deja vu (29%) and clairvoyance (24%), and there was a very small amount of belief in the Loch Ness Monster (14%), Sasquatch (13%), ghosts (12%) and witches (10%). Most people were willing to state an opinion, with a consistently low pattern of no opinion responses for all phenomena which did not seem to differ among them except perhaps for a slightly lower uncertainty

* Tables are at the end of this article.

for religious phenomena. The homogeneity of belief was consistent among the religious and other phenomena with high or moderate levels of belief, and high among those of low belief, as seen in the standard deviations.

It was hypothesized that belief in the various types of paranormal phenomena would vary considerably, with those associated with established religion (angels, devils and life after death) different from the other types of paranormal phenomena. Table 2 presents a Pearson's zero order correlation matrix of all these beliefs which supports this contention, with high correlation of $r=.70$ between belief in angels and devils, $r=.42$ between angels and life after death and $r=.39$ between devils and life after death. Additional correlations which were substantially higher than most others in the table were between the Loch Ness Monster and Sasquatch ($r=.58$); and ESP and precognition ($r=.46$), déjà vu ($r=.41$), and clairvoyance ($r=.45$). All of the relationships were positive, showing believers in one type of paranormal phenomena were likely to be believers in another. Many correlations were weak, indicating that individuals are not simply believers in all types of paranormal phenomena, but rather discriminated between them.

To further understand these beliefs, a factor analysis was done in an attempt to assess meaningful latent dimensions in types of paranormal belief. Three factors were produced by the principal components factor analysis. Oblique rotation was chosen over orthogonal because of its better representation of empirical reality and our assumption that underlying factors would be correlated after rotation. Factor correlations were .33 between factor 1 and 2, -.59 between 1 and 3, and -.21 between 2 and 3, as seen in Table 3 which also presents the factor pattern scores. Factor 1 has relatively low factor loadings for the religious variables of angels (-.128), devils (-.03) and life after death (.17) and high ones for ESP (.67), precognition (.66), déjà vu (.61), clairvoyance (.63) and astrology (.30). These types of paranormal belief are all extra-sensory, psychic types of phenomena, and we may label this factor the "psychic" dimension of belief. Factor 2 has high loadings for the religious beliefs in angels (.91), devils (.80) and life after death (.44) and low loadings for all other beliefs. Clearly this is a "religious" dimension of paranormal belief. Finally, the third factor has high loadings for belief in the Loch Ness Monster (.64) and Sasquatch (.84) and moderate loadings for ghosts (-.25) and witches (.23). While not as clearly defined as the other two factors, the common denominator of these types of paranormal belief is that they are nonreligious beings or beasts not recognized by our scientific or religious institutions. Thus we can label the third dimension the "other beings" factor. These three underlying dimensions show the latent structure of belief in paranormal phenomena, reducing the twelve types of phenomena originally assessed to three underlying explanations of belief. There is a high correlation between factors 1 and 3 (-.58) and three phenomena, ghosts, witches and astrology, load highly on both factors. These three are not clearly psychic nor other beings in people's perceptions, occupying a somewhat ambiguous role in our classification

scheme.

We may finally examine the relationship between belief in these phenomena and demographics of the believers in light of the three underlying dimensions revealed by the factor analyses. Sex, race, and age are presented in table 4. Females are more likely to believe in the religious phenomena, although this is only significant for angels and life after death. Women are also more likely to report belief in all of the psychic variables, but again only two, ESP and astrology, are significant. Males report slightly more belief in all the other beings, but they are not significantly different than females in this pattern.

Partially because of the small number of blacks (159), there are few significant differences between races. No significant differences exist for the religious beliefs, or most of the other beings. There is an 11.1% difference in belief for the Loch Ness Monster, perhaps because the alleged beast is only part of Anglo-Saxon heritage. Among the psychic variables, blacks believe significantly less in precognition and *deja vu*, and over 13 percent more in astrology.

Age is not a significant determinant of belief in our religious phenomena, providing support for the fact that they are a different type of belief than all of the others, which are significantly related to age in consistent inverse relationships. Belief in religious unexplained phenomena does not decline with age, possibly through continual reinforcement by religious institutions. In contrast, young people tend to be strong believers in psychic phenomena, and to a lesser extent in other beings, but such belief declines greatly with age. This may be due to a greater participation of youth in popular culture and its alternative world views and fad beliefs. With maturity they become increasingly involved in the dominant belief systems and may abandon earlier commitment to these kinds of unexplained phenomena.

There are significant educational differences in belief on all three dimensions, seen in table 5. Quasi religious beliefs are significantly related to education for angels and devils in an inverse relationship, while belief in life after death is not significantly related. Psychic beliefs are directly related to education, with very significant relationships for ESP, precognition, *deja vu* and clairvoyance. There is an anomaly in these otherwise fairly linear positive relationships in the consistently lower belief of college graduates. This "tailing-off" of the end of the curve shows skepticism in the highest educational groups. The belief in the other beings is significant for Sasquatch and the Loch Ness Monster in a generally positive relationship, again with a "tailing-off" of beliefs for college graduates. Other beliefs were not significantly related to education except for astrology, which had an inconsistent, somewhat curvilinear pattern. These relationships of belief with education are consistent in distinguishing the three

previously identified dimensions, especially in differentiating the religious, which is inversely related to education, from the other two, which are directly related.

Turning to marital status, as seen in table 6, we can see that belief in the religious factors is only significant for angels, where it is low for single and divorced people, higher for married individuals, and highest for the widowed and separated. Belief in other beings is consistently low for the widowed and married, high for singles, and varying for divorced and separated. The psychic factors are also consistently low in belief for widows and married people, high for singles and usually divorced people, and lower for those who are separated. These findings do show the difference in the dimensions, but may be more a consequence of age differences than qualities inherent in marital status itself.

Discussion

In this analysis twelve different unexplained phenomena were examined with respect to how people believed in them.* While it was found that believers in any one type were likely to believe in others, the relationships among beliefs were not strong enough to assert that people were either general believers or nonbelievers. A factor analysis revealed three underlying dimensions in unexplained beliefs: religious, psychic, and other beings. Angels, devils and life after death are part of the dominant religious beliefs of our culture, and may be seen as reinforced by the religious system.

Other beliefs in unexplained phenomena are not religious in origin, and are not closely related to religious beliefs. They may be seen as related to the myths of science in our culture. Rather than being seen as anti-science, in opposition to dominant scientific paradigms, they may be interpreted as going beyond what modern science can explain. Believers do not necessarily renounce science, but only see that much is as yet undiscovered and believe that the scientific paradigm is incomplete and does not account for much of the world which is unknown. In a sample of college students, Bainbridge (1978) found an insignificant but positive relationship between favorable attitudes toward science and belief in the popular myth of ancient astronauts. On the other hand, Hartman (1976) found that the belief system of readers of the occult journal Gnostica included the rejection of science as a major source of truth.

There are two dimensions in this extra-scientific set of beliefs, as seen in the two nonreligious factors of our data. The first involves psychological aspects of life such as interpersonal communication, perception, and mental patterns as seen in ESP, precognition, déjà vu and clairvoyance. Beliefs in these phenomena involve unexplored and unexplained aspects of our selves and our minds which are unusual happenings and experiences that seem to go beyond the scope of scientific explanation.

Belief in these other processes and other laws which are entrenched in popular culture may be contrasted with beliefs in other beings. Rather than being seen as undiscovered aspects of our own minds, beliefs

* This is an extension of the earlier work of Emmons and Sobal (1981a,1981b).

in other beings appeal to a sense of unexplored places. Belief in other species or other forms of life assumes that there are environments or sites which have not been fully investigated by science. Belief in the Loch Ness Monster or Sasquatch suggests that people have a strong sense of frontier, holding on to the notion that oddities of biology exist in remote areas, waiting for scientists to discover and explain them. Unidentified flying objects (UFO's) were not included in this investigation, but would probably be seen as part of this dimension of belief in other beings, as undiscovered organisms in areas too remote for science yet to have penetrated. Yet UFO's are often ascribed with psychic types of qualities, and would probably also be heavily involved with the psychic dimension of belief. Witches and ghosts also are believed to be other beings which are not yet fully understood in the places where they are purported to exist, and also possess psychic types of qualities. Belief in monsters, either natural (vampires, werewolves) or manmade (such as Frankenstein's monster, living mummies, or other man/machine creations such as robots), seems to also be explainable in terms of the other-beings dimension in conjunction with the psychic dimension.

We may also interpret these dimensions of belief in light of their use by people in relating to their world. Originally distinguished by Emile Durkheim (1947) and Bronislaw Malinowski (1948), "Magic..., science and religion are a 'three-cornered constellation' ...which are distinct but interconnected modes of adjustment which enable men to meet uncertainty, attain rational mastery of their environment, and deal with problems of meaning respectively" (Fox 1974:p.232). Our dimensions of belief are in line with this troika of methods for dealing with uncertainty in the world. Belief in psychic events reflects an uncertainty about self and mind, and a hope that new types of mental magic are soon to be discovered. Belief in other beings can be seen as uncertainty about the vastness of the world and the scope of the natural environment, with science having the potential for incorporating these new discoveries into our worldview. Finally, belief in religious unexplained phenomena expresses uncertainty about cosmology and the meaning of life, for which institutionalized religion and its myths provide explanations.

These patterns of belief are consistent with the demographics of belief in each dimension. If females are more involved in religious institutions, they should therefore be more likely to believe in the religious phenomena. They may also be more concerned with interpersonal, psychological aspects of life than males, and believe more in psychic factors. In contrast, males are more likely to be oriented to the frontier and the outdoors, and believe more in other beasts and beings. The inverse relationship of age and belief in psychic and other beings type of phenomena is consistent with the questioning of established beliefs, openness to alternative conceptions of the world, and lack of socialization and integration into established institutions on the part of youth.

Education was inversely related to belief in the religious phenomena, which is in line with the declining religious involvement of more highly educated individuals. Other beliefs were positively related to education, in contrast to the religious beliefs. This may

be more an indicator of awareness than education, as exposure to many of these phenomena in our culture comes through written material and plays, stories, etc. This interpretation is supported by the drop in belief for college graduates, who have not only been exposed to material on these phenomena but are more intellectually prepared to debunk them or offer scientific paradigms as alternative explanations. Examination of the patterns by marital status showed high levels of belief in the religious phenomena for widows and married people, who are older and usually more religious. The single, divorced and separated are younger and less tied to religion, and they reported lower belief levels in religious phenomena. The reverse was true for all the nonreligious phenomena, with the married and widowed showing low belief and other unmarried high. While age may account for differences between married, single and widowed people, the generally lower belief in religious phenomena by divorced and separated individuals does suggest that a lack of involvement in marriage as an institution is associated with patterns of belief.

These findings may have important implications for parapsychological research. Belief or nonbelief as a phenomenon may have an impact upon performance in tests of psychic power. The knowledge gained from understanding the demographics of belief may allow researchers to more fully take belief patterns of subjects into account. Further investigation of belief may enhance the ability to predict performance.

In conclusion, unexplained phenomena are not of a single type. They are related to myths and paradigms in our culture, and should be interpreted in light of these patterns. Because of their potential influence on people's lives and interpretations of the world, belief in them deserves more attention. Further research should examine a wider range of unexplained phenomena. The origins of belief and intensity and salience of belief in these things should also be explored.

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Table 1
Belief in Twelve Unexplained Phenomena

Phenomena	Believers % (N)	Nonbelievers % (N)	No Opinion % (N)	Belief Score*	Standard Deviation
Angels	53.5 (831)	42.6 (661)	3.9 (61)	1.44	.50
Devils	39.0 (606)	55.8 (867)	5.2 (80)	1.58	.49
Life After Death	62.5 (970)	33.4 (519)	4.1 (64)	1.34	.48
Loch Ness Monster	14.0 (217)	79.1 (1229)	6.9 (107)	1.84	.36
Sasquatch (Bigfoot)	13.1 (204)	79.9 (1241)	7.0 (108)	1.85	.35
Witches	9.7 (151)	84.0 (1304)	6.3 (93)	1.90	.31
Ghosts	11.7 (182)	82.4 (1279)	5.9 (92)	1.88	.33
Astrology	29.0 (451)	64.8 (1006)	6.9 (96)	1.69	.46
Extra Sensory Perception (ESP)	50.4 (783)	44.9 (693)	4.6 (72)	1.47	.50
Precognition	37.2 (577)	57.4 (891)	5.5 (85)	1.61	.49
Deja Vu	29.4 (456)	64.5 (1002)	6.1 (95)	1.69	.46
Clairvoyance	23.5 (365)	69.3 (1077)	7.1 (111)	1.75	.43

* Belief score is the average belief when believers equalled 1 and nonbelievers = 2. A lower score means more belief. The Standard Deviation is calculated for this score.

Table 2
Pearson Correlation Coefficients of Unexplained Phenomena

	Angels	Devils	Life After Death	Loch Ness Monster	Sasquatch (Bigfoot)	Witches	Ghosts	Astrology	ESP	Precognition	Dejavu	Clairvoyance
Angels	1.00 S=.001	.710 S=.001	.430 S=.001	.090 S=.001	.100 S=.001	.180 S=.001	.080 S=.001	.200 S=.001	.140 S=.001	.140 S=.001	.200 S=.233	.100 S=.001
Devils		.700 S=.001	.1.00 S=.001	.390 S=.001	.110 S=.001	.140 S=.001	.260 S=.001	.180 S=.001	.180 S=.001	.150 S=.001	.190 S=.001	.080 S=.001
Life After Death			.430 S=.001	.390 S=.001	1.0000 S=.001	.130 S=.001	.120 S=.001	.170 S=.001	.170 S=.001	.140 S=.001	.210 S=.001	.210 S=.001
Loch Ness Monster				.090 S=.001	.110 S=.001	.130 S=.001	1.0000 S=.001	.590 S=.001	.290 S=.001	.350 S=.001	.190 S=.001	.330 S=.001
Sasquatch (Bigfoot)					.090 S=.001	.140 S=.001	.120 S=.001	.590 S=.001	1.0000 S=.001	.300 S=.001	.360 S=.001	.200 S=.001
Witches						.180 S=.001	.260 S=.001	.170 S=.001	.290 S=.001	1.0000 S=.001	.420 S=.001	.250 S=.001
Ghosts							.080 S=.001	.180 S=.001	.170 S=.001	.350 S=.001	1.0000 S=.001	.230 S=.001
Astrology								.200 S=.001	.180 S=.001	.140 S=.001	.190 S=.001	1.0000 S=.001
ESP									.200 S=.001	.150 S=.001	.210 S=.001	.330 S=.001
Precognition										.150 S=.001	.190 S=.001	.210 S=.001
Dejavu										.020 S=.233	.080 S=.001	.110 S=.001
Clairvoyance											.100 S=.001	.170 S=.001

S=Significance

Table 3
Factor Analysis of Unexplained Phenomena Beliefs

Factor Pattern	Factor 1	Factor 2	Factor 3
Angels	-0.128	0.919	0.000
Astrology	0.297	0.139	-0.063
Precognition	0.676	0.023	0.044
Dejavu	0.618	-0.116	-0.047
Witches	0.270	0.153	-0.232
ESP	0.675	0.003	0.012
Sasquatch	-0.069	0.001	-0.847
Loch Ness Monster	0.011	-0.026	-0.645
Ghosts	0.373	0.020	-0.254
Devils	-0.036	0.801	-0.045
Life After Death	0.170	0.435	0.047
Clairvoyance	0.624	0.020	-0.003

Factor Correlations

	Factor 1	Factor 2	Factor 3
Factor 1	1.00000	0.33089	-0.58779
Factor 2	0.33089	1.00000	-0.21447
Factor 3	-0.58779	-0.21447	1.00000

Table 4

DEMOGRAPHIC ATTRIBUTES OF BELIEVERS

Overall Belief %	Sex		Race		Age					
	Male %	Female %	White %	Black %	18-29	30-39	40-49	50-64	65+	
Angels	55.7	52.4*	58.9*	55.4	59.1	53.2	55.7	57.8	55.2	57.9
Devils	41.1	40.3	42.0	40.7	44.7	43.8	41.9	42.9	38.2	38.5
Life After Death	65.1	62.5*	67.7*	65.6	62.7	63.4	67.0	63.7	68.2	63.9
Loch Ness Monster	15.0	15.6	14.4	16.3**	5.2**	25.0***	18.3***	14.9***	8.9***	5.6***
Bigfoot (Sasquatch)	14.1	15.1	13.2	14.8	8.4	26.4***	16.7***	12.2***	9.2***	2.6***
Witches	10.4	10.8	10.0	10.5	8.9	15.0***	12.9***	12.9***	5.2***	4.4***
Ghosts	12.5	12.6	12.3	12.2	14.0	20.3***	16.4***	12.5***	6.9***	4.0***
Astrology	31.0	28.3*	33.5*	29.5**	43.3**	37.7**	30.8**	32.1*	25.9**	27.1**
ESP	52.9	48.7*	56.9**	54.8	38.0	65.3***	59.0***	55.1***	49.2***	31.9***
Precognition	39.3	36.8	41.7	40.4*	30.6*	52.6***	43.4***	36.3***	34.1***	25.6***
Deja vu	31.3	30.4	32.2	32.9***	17.6***	50.8***	32.4***	30.9**	23.9***	10.9***
Clairvoyance	25.3	24.4	26.2	26.1	19.2	32.7***	26.4***	29.9***	22.3***	13.8***
	N=736	N=756	N=1326	N=159		N=372	N=282	N=232	N=315	N=278

* chi square p < .05

** chi square p < .01

*** chi square p < .001

(asterisks are printed for all categories of a significant variable)

Table 5
Education and Belief in Paranormal Phenomena

	Overall % belief (N)	0-4th grade	5-7th grade	8th grade	9-11th grade	High School Graduate	Technical/Trade Business School	Some College	College Graduate
Angels****	55.8 (831)	75.7 (28)	65.7 (46)	62.8 (59)	60.2 (145)	58.4 (294)	65.9 (54)	46.2 (116)	42.0 (89)
Devils***	41.2 (606)	54.1 (20)	50.0 (35)	46.2 (43)	41.1 (99)	44.9 (221)	46.9 (33)	34.6 (35)	30.8 (65)
Life After Death	65.2 (970)	64.9 (24)	57.6 (38)	67.7 (65)	59.0 (144)	67.2 (336)	65.1 (54)	67.6 (169)	66.4 (140)
Loch Ness Monster***	15.0 (217)	2.8 (1)	0.0 (0)	5.8 (5)	12.0 (28)	17.5 (85)	18.3 (15)	20.2 (49)	16.2 (34)
Sasquatch (Bigfoot)**	14.1 (204)	8.1 (3)	6.0 (4)	8.0 (7)	12.8 (30)	15.5 (75)	19.5 (16)	19.3 (47)	10.6 (22)
Witches	10.4 (151)	10.8 (4)	4.4 (3)	7.7 (7)	8.8 (21)	10.3 (50)	14.5 (12)	14.3 (35)	9.2 (19)
Ghosts	12.5 (182)	11.1 (4)	11.6 (8)	11.1 (10)	12.1 (29)	11.2 (55)	10.8 (9)	16.9 (41)	12.5 (26)
Astrology**	31.0 (451)	29.4 (10)	31.8 (21)	22.5 (20)	39.6 (95)	32.4 (159)	33.7 (28)	30.6 (75)	20.8 (43)
Extra Sensory Perception (ESP)	52.9 (783)	20.0 (7)	17.6 (12)	32.6 (28)	44.3 (108)	55.3 (276)	63.9 (53)	50.4 (178)	57.3 (121)
Precognition****	39.4 (577)	21.6 (8)	19.7 (13)	19.5 (17)	33.5 (81)	40.8 (200)	42.0 (34)	52.8 (133)	43.1 (91)
Deja Vu***	31.3 (456)	5.7 (2)	10.6 (7)	12.5 (11)	20.3 (48)	31.2 (154)	43.2 (35)	51.0 (126)	33.9 (73)
Clairvoyance****	25.3 (365)	8.8 (3)	4.5 (3)	17.2 (15)	19.6 (47)	24.9 (121)	34.2 (27)	35.0 (85)	31.1 (64)

Chi Square significance * p < .05

** p < .01

*** p < .001

**** p < .0001

Table 6

Marital Status and Belief in Unexplained Phenomena

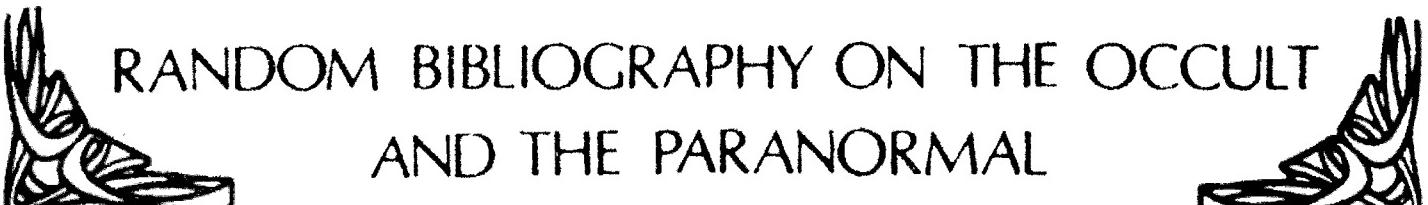
	Overall Beliefs % (N)	Married	Marital Status	Single	Divorced	Separated	Widowed
Angels**	55.8 (831)	56.8 (586)	46.7 (113)	50.7 (36)	66.7 (14)	65.6 (82)	
Devils	41.2 (606)	42.3 (429)	36.6 (87)	43.1 (31)	57.1 (12)	37.6 (47)	
Life After Death	65.2 (969)	65.2 (666)	64.5 (153)	58.6 (41)	72.7 (16)	68.3 (88)	
Loch Ness Monster**	15.0 (217)	14.1 (141)	21.6 (51)	17.4 (12)	14.3 (3)	8.3 (10)	
Sasquatch (Bigfoot)*	14.1 (204)	13.9 (138)	19.2 (46)	13.4 (9)	19.0 (4)	5.8 (7)	
Witches***	10.4 (151)	7.7 (77)	18.3 (44)	20.0 (16)	22.7 (5)	8.8 (11)	
Ghosts**	12.5 (182)	10.7 (107)	19.2 (46)	20.0 (14)	22.7 (5)	8.0 (10)	
Astrology***	31.0 (451)	27.9 (278)	35.0 (84)	44.3 (31)	52.4 (11)	37.3 (47)	
Extra Sensory Perception (ESP)****	52.9 (783)	50.5 (513)	62.6 (154)	69.4 (50)	45.5 (10)	45.2 (56)	
Precognition****	39.4 (577)	36.2 (364)	48.4 (119)	60.6 (43)	45.0 (9)	33.9 (42)	
Deja Vu***	31.3 (456)	28.6 (286)	43.3 (106)	55.9 (38)	28.6 (6)	16.4 (20)	
Clairvoyance**	25.3 (365)	23.8 (235)	32.4 (78)	33.3 (23)	40.0 (8)	17.4 (21)	

Chi Square significance: * p < .05

** p < .01

*** p < .001

**** p < .0001



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ERRATA

Errata to ZEETETIC SCHOLAR #8:

1) The top of page 52 should have started with the missing line:

"Probably all can agree that Carl Sagan has been the principal"

2) The book review by D.H. Saklofske was co-authored by Ivan W. Kelly.

MYSTERY MEN FROM HOLLAND, II: The Strange Case of Gerard Croiset

PIET HEIN HOEBENS

With few exceptions, educated citizens of the Netherlands have always tended to dismiss their former fellow-countryman Peter Hurkos as a typical Hollywood character: good enough for gullible Californians, but far too implausible for sober Dutchmen. Gerard Croiset, however, is a different kettle of fish. This remarkable clairvoyant, who died in July 1980, was taken fairly seriously in his native country, even by many persons who otherwise professed a strong disbelief in the occult. The case of Gerard Croiset is a strange and complex one. To a certain extent, he is a genuine challenge to the skeptic. My private belief is that he had no more than five senses. This opinion, however, may be strongly influenced by what Dr. Beloff would term my "metaphysical predilection" for the non-existence of psi.

On the basis of the evidence which others and I have uncovered,¹ I may certainly urge the reader, at the very least to suspend belief in Croiset's paranormal powers. Even the most copper-bottomed of "proofs," I have found, are not above suspicion. It is true that Gerard Croiset, virtually alone among the internationally famous psychic detectives, has been vouched for by a prominent parapsychologist. However, the work of Dr. W.H.C. Tenhaeff has now been shown to be flawed in unsuspected ways. It seems perfectly rational to expect that the entire Croiset phenomenon in due time will be explained in terms of erroneous reporting, personal validation, coincidence and fraud. Yet I prefer not to draw premature conclusions. I must point out, for example, that Mr. George Zorab, who for years has drawn the attention of his fellow psychical researchers to serious shortcomings in the published evidence² and who from personal experience is convinced that Croiset was at least a part time cheat, yet continues to feel that the subject of this article was a genuine sensitive. The question as to whether Croiset had any powers of extrasensory perception (if such exist) will not be settled here. I will restrict myself to presenting further reasons for extreme caution in accepting the proponents' reports at face value. As in my earlier article on Hurkos, I will critically examine supposedly respectable accounts of the psychic's feats as they have been published in English.

MYTHS

My statement that Croiset "was taken fairly seriously in his native country" should not be misunderstood. It is true only if we compare his local reputation with Hurkos' or Dykshoorn's. Contrary to popular mythology abroad, however, Croiset most certainly was not the psychic stand-by of the Dutch police. Although incidental cases of co-operation are known, the police in the Netherlands have traditionally been skeptical of paranormal detectives. Reports published abroad often convey a highly misleading impression. With sensationalist

newspapers such as National Enquirer this is to be expected. However, the mis-information is not restricted to the tabloids.

Mr. Roy Stemman, co-editor of the now defunct magazine Alpha and an experienced reporter on the occult, furnishes a typical example in his 1981 article "Croiset: The Psychic Detective."³ This article is accompanied by a photograph showing Dr. Tenhaeff, the clairvoyant and a uniformed individual whom Mr. Stemman identifies as "the Utrecht chief of Police." According to the caption beneath, "they were a regular team, Croiset helping the police in their search for missing persons and Professor Tenhaeff monitoring the clairvoyant's progress." Untrue, I'm afraid. The uniformed gentleman is not the Utrecht chief of police. And neither did Croiset, Tenhaeff and the Utrecht chief of police form a "regular team." In fact, the successive Utrecht chiefs of police have been notoriously skeptical of Gerard Croiset. One of them, Mr. Th. van Roosmalen, was the author of one of the most devastating "debunkings" of that psychic ever published.⁴ As late as 1980, the official spokesman of the Utrecht corps told me that none of Croiset's attempts to locate missing persons or solve crimes in his home town had ever been successful.

Mr. Stemman's article concludes: "Gerard Croiset died on 20 July 1980, at the age of 71. But the records on file at Utrecht University will continue to intrigue and baffle scientists for many years to come." I am afraid that scientists who wish to be intrigued and baffled will come to Utrecht University in vain. The whereabouts of Dr. Tenhaeff's celebrated files are a mystery, even to Dr. Tenhaeff's successor as "special Professor of Parapsychology."⁵ Some backstage information enables me to make an educated guess as to what had happened to these precious documents. In any case, they are not available for examination. The desire of certain persons to avoid further embarrassment may have contributed to this sad state of affairs.

TENHAEFF AND POLLACK

As I have argued elsewhere, the decisive factor in Croiset's rise to international fame has probably been the fact that his powers of extrasensory perception have been vouched for by a prominent psychical researcher. Professor Doctor Wilhelm Heinrich Carl Tenhaeff enjoyed a considerable reputation, especially on the European continent. He held the first chair of parapsychology ever established at a major western university. His German colleague Professor Hans Bender has praised him as one of the great pioneers of parapsychology. When Tenhaeff died in July 1981, Professor Andreas Resch, the catholic parapsychologist of the Innsbruck Imago Mundi institute, wrote an extensive obituary for the German magazine Esotera,⁶ entitled "Search for the Truth," in which the deceased was listed with the "great researchers of the soul in the history of psychology and parapsychology." The Parapsychology Review called him a "noted world figure in parapsychology."⁸ Given the chief chronicler's credentials, it is hardly surprising that writers on the occult, particularly if they were both foreigners and "believers," were only too happy to accept at face value what they were told about "The Dutchman with the X-ray Mind." After all, there was a body of "official" evidence, collected, verified, and published by a respected University Professor.

To a certain extent the book Croiset the Clairvoyant by the American journalist Jack Harrison Pollack⁹ forms part of this official evidence, as it was written under the personal supervision of Tenhaeff, who double-checked the manuscript and who openly endorsed the book - which has been translated into German and French. Croiset the Clairvoyant is an important source, as few of Tenhaeff's own publications are available in English. In this article, I will critically examine two prize cases as described in the book. In addition, I will analyse two important cases of which reports by Tenhaeff himself have been published in this language.

CONVERSATION WITH A TEACHER

The claim (pp 108-109 of the Bantam edition of Croiset the Clairvoyant): On February 21, a seven-year old child disappeared in Utrecht. Police could find no trace of him. Three days later Croiset, then living in Enschede, was telephoned by the child's schoolteacher, Miss H.M. "I have a clear picture of the child," the psychic is reported to have said, "I see military barracks and a shooting range. There is grass around it. In the grass is a small hill. There I see water also. In this water, the child fell and drowned. He is there now. His body will be found by a man in a small boat. This man wears a colored band around his cap. When you come from Enschede toward Utrecht, it is on the left side of the road."

On March 1, Tenhaeff asked Croiset whether he had more information. The clairvoyant answered without hesitation: "Yes. As I told his teacher, the child had definitely drowned in the water by Gort de Bilt (Outside Utrecht). His body will soon be found." On March 5, the boy's mortal remains were discovered "precisely where Croiset had said" by a skipper of the harbor service who wore a colored band around his cap.

The claim is of some interest, as it is the only report I have been able to locate concerning a supposedly successful attempt by Croiset to solve a police case in the major town of Utrecht. Curiously, it must have happened practically under my two-year old nose, as at the appropriate time, I lived a few hundred yards from Fort de Bilt.

My investigations, in 1981, soon revealed that vital bits of information are missing from Pollack's account. The same case is reported by Tenhaeff in the Dutch Tijdschrift voor Parapsychologie.¹⁰ This report mentions a few relevant details that are lacking in Croiset the Clairvoyant. First: the schoolteacher, Miss H.M., knew Croiset well. Second: before the phonecall on February 24, Miss H.M. had already called Croiset twice. On February 22, the clairvoyant had told her that "there is no reason to worry." He had added that he would be ready to go and search for the child in case he hadn't surfaced by next Saturday. On Friday night, Mrs. Croiset told Miss H.M. that her husband, who was sleeping, now was "less optimistic." He had "the impression that the boy was no longer alive." Tenhaeff's account of the telephone conversations of February 24 and March 1 essentially confirms Pollack's.

There is something odd about this story. If - as is claimed - Croiset on February 24 knew exactly where the boy was at that moment, then why was the dismal discovery not made until March 5? Is it conceivable that, for ten days, no one would have searched the location indicated by the psychic? Strangely enough, neither Pollack's report nor Tenhaeff's mentions any attempt on the part of either the schoolteacher or the Professor to relay this information to the police.

On September 2, 1981, the Vice Superintendent of the Utrecht police wrote me to say that the department's files do not go back as far as 1951. Thanks to Dr. F. Brink,¹¹ I was able to contact Mr. Wielinga, a retired police officer who, in February and March 1951, was on duty in Utrecht. Mr. Wielinga distinctly remembered the tragic incident. He did not remember that Croiset or any other psychic¹² had furnished useful information to the authorities. He strongly doubted the story.

A search through contemporary newspaper files dissolved whatever mystery may have remained to this point. Both Pollack and Tenhaeff fail to mention the important fact that the victim, Appie Verbeek, lived in the Gildstraat in the immediate vicinity of Fort de Bilt, one of several military installations in the eastern part of Utrecht. Shortly before disappearing, the boy had been seen walking in a nearby street. In the area there is a canal, known as the Biltse Grift, which runs from the Griftpark to De Bilt, passing the barracks of the Fort. When a seven-year old child disappears and does not return for several days, the odds are that he is dead and that his remains are not far from where he was last seen. Any location in the close vicinity of the Gildstraat would also be in the close vicinity of Fort de Bilt. As for Pollack's claim that the body was found "precisely where Croiset had said": the body was not found in the waters by Fort de Bilt but in the Biltse Grift next to the Museum Bridge, inside Utrecht, about half-way between the Gildstraat and the Fort. Nearer-by are several highly visible landmarks such as a graveyard, a rotunda, a park and two palace-like buildings. A "precise" description would have included the elements "bridge" and "graveyard," not the Fort, which is outside town. I do not know whether the skipper who found the body wore a cap with a colored band around it. The newspaper reports do not mention this detail and neither does Tenhaeff. It seems unlikely that the Professor would have accidentally overlooked this "hit." Suffice it to say that caps with colored bands are far from rare in Holland. To summarize: Croiset, when consulted by a person he knew well, first said that the child was alive. He changed his mind only when the boy did not return after a couple of days and the police told the press that an accident was likely. He later mentioned a landmark in the immediate vicinity of where the child had lived. Ten days later the body was found at a different location. It is not entirely clear to me how this case can honestly be presented as an example of successful psychic detection. It is important to note that Tenhaeff saw and approved the manuscript of Croiset the Clairvoyant.

A GERMAN CHILD DISAPPEARS

Summary of Pollack's account (pp 113-115 of Croiset the Clairvoyant): Late in 1957 five-year old Bernard Schlegel from Bustehude, Germany,

vanished. The police were inclined to think that the child had been kidnaped and possibly murdered. In any case, there was a "general belief that the child had not drowned." Dr. Hans Bender, parapsychologist at Freiburg University, suggested that Croiset be consulted. In co-operation with the police Heinz Metzger, journalist with the Hamburger Abendblatt, visited Croiset in Holland in late January. The psychic "had heard nothing of the boy's disappearance" yet knew immediately what the reporter had come for. Croiset is quoted to have said: "This child has something to do with a kiosk (a sort of magazine stand with open sides and a roof, usually of canvas). I see a shop in the neighborhood. It has a striped awning with a tear on the lower right side ... The child is dead. I have no doubt. The child must have drowned." Herr Metzger told him that the "Oste river" which runs through Buxtehude, had been dragged but that nothing had been found. Croiset then described a factory, drew a sketch and stated that the body was lying about 400 meters "behind the factory." The police would be able to find him, but it would take a lot of time.

About three weeks later, the body was found in the "Oste river," "near the factory Croiset had described and corresponding to his sketch." Pollack concludes: "So once again, on a case he had known nothing about, Gerard Croiset's paranormal pictures led to the discovery of the body, the more remarkable in the face of a general belief that the child had not drowned. The German police's faith in the Dutch sensitive's powers became stronger when they checked his impressions and found them correct. One detail that deeply impressed them was Croiset's specific image of the striped awning, torn on the lower left side."

In 1981 I collected a considerable amount of information concerning this case. Some of this was given in confidence, but what I am at liberty to make public is sufficient to demonstrate that Pollack's report is misleading in the extreme.

The report suggests that there was an "official" element in Croiset's performance as the psychic was consulted on the advice of Dr. Bender and in co-operation with the police. In fact, Dr. Bender has stated that he heard of the case only afterwards. And in a letter to DEGESA (The German Society against Superstition) dated February 18, 1958, the Landekriminalpolizei points out that the only witness of the consultation in Utrecht had been Herr Metzger. The police "could not confirm whether Mr. Croiset's statements were correct and how they were arrived at."

Heinz Metzger, a crime reporter, had covered the case of Bernard Schlegel from the beginning. In an article in Hamburger Abendblatt he states: "I told Croiset all I knew concerning the boy. I described to him all possibilities and outlined all surmises." Only then did Croiset mention the kiosk, the journalist replying that, indeed, "the spoor had ended at the station's kiosk."¹³ No tape-recording of the entire conversation exists. It would have been interesting to check in how far this was an instance of information furnished by an unwitting client being fed back as a "telepathic impression." Herr Metzger's statement about having previously told all he knew suggests a non-miraculous explanation. Of course, the possibility that Croiset had been informed of the case prior to Herr Metzger's visit should not be overlooked. The boy had been missing since Christmas. Numerous articles had appeared in the German press.

Pollack's claim about the "general belief that the child had not drowned" is simply false. The Schlegel boy lived about 50 yards from the river Este (not Oste) and the police had assumed from the start that he had fallen into the water. This is stated by Pelz¹⁴ and is confirmed in Metzger's report in Hamburger Abendblatt of January 28, 1958.

On November 16, 1981, I had a revealing telephone conversation with Herr Metzger, presently chief editor of the major Berlin daily newspaper B.Z. Pollack's chief witness surprisingly turned out to be a complete skeptic as to Croiset's clairvoyant powers. He explained the "hits" not accounted for by the possibility of prior information as the result of post factum interpretation of an ambiguous psychic reading. Finding matches between some of Croiset's statements and actual Buxtehude locations proved easy, due to the vagueness and generality of the former.

The striped awning "with a tear on the lower right side"¹⁵ is a case in point. According to Pollack, Croiset had mentioned a shop. From the January 27 report in Hamburger Abendblatt we learn that the clairvoyant had in fact referred to a pub. Not surprisingly, there was a pub near the station. The awning, however, belonged to a near-by shop. The police had little reason to be "deeply impressed." Awnings are a common sight in European towns and many are torn at the sides. As a photograph published by Pelz¹⁶ clearly shows, the Buxtehude awning had tears on both sides, not just the right one. (Hamburger Abendblatt on January 28 published only the right half of the same picture.) The shop with the awning had played no role in the drama. The odds against chance entirely depend on whether or not it is likely that a slightly damaged awning is found somewhere in the central area of a small German town. The worst error in Pollack's version is the claim that the body was found "near the factory Croiset had described and corresponding to his sketch." "Absolutely untrue," Herr Metzger told me on November 16. The real facts are these. Two branches of the river Este flow through Buxtehude. One is known as the "Gestaute Arm," the other as the "Umfluter." As is apparent from the original reports in Hamburger Abendblatt, Croiset had finally decided that the body must be lying in the "Gestaute Arm," about 400 meters behind the factory. This is where a dam closes off the branch. And this is where the police suspected that the body would be. A search had been impossible due to the fact that the water was frozen. Both Croiset and the police were wrong. Bernard Schlegel's body was found in the "Umfluter," two and a half kilometers from the factory.

To summarize: Gerard Croiset had simply confirmed what everybody had assumed from the start. His only original contribution to the solution of the case consisted of a guess that proved to be dead wrong.¹⁷

IN TENHAEFF'S OWN WORDS

One of the few authoritative English language publications on Croiset's work as a psychic detective - apart from Pollack's book - is the article "Aid to the Police" which Tenhaeff wrote for Tomorrow, the "World Digest of Psychical Research and Occult studies" published by Eileen Garrett. The article¹⁸ is based on a paper which Tenhaeff read at the Parapsychology Foundation's First International Conference on Parapsychological Studies held at Utrecht State University in the summer of 1953. This was an important occasion for Tenhaeff, for it provided him with one of the first opportunities to inform his colleagues of the results of his work with his favourite sensitive.

He begins by pointing out that the consultation of clairvoyants by the police "is a more complex affair than many an uninitiated may assume." Often, the information provided by such psychics did not advance police investigation as such, but still "proved interesting in terms of para-psychological research." "Nevertheless, some cases can be cited where the contribution of Mr. Croiset was of concrete use to the police and the courts of law." Of the examples he then describes, three stand out because 1) they concern attempts to solve crimes by ESP; 2) they seem fairly striking, and 3) they are reported in sufficient detail to enable the critical investigator at least to identify the incidents to which they relate.

One of these cases will not be dealt with in this article. It is the celebrated affair of the Wierden Hammer Assault, discussed by Hansel.¹⁹ After reading Pollack's account of this case, Hansel made inquiries with the Wierden authorities and was told that Croiset's efforts had been of no use to the police. A complete analysis of this case and of the controversy surrounding it would require far too much space but may be published separately in the future. Suffice it to say at this point that the ESP hypothesis is not supported by the facts.

The other two cases, however, have never before been the subject of critical examination.

THE COFFEE SMUGGLERS

A summary of the account in the Tomorrow article (pp. 13-14): On April 11, 1953, a Mr. A. M. Den Hollander, an official of the Customs' Department at Enschede, had provided Tenhaeff with an extensive report of a meeting with Croiset on the previous November 10. Den Hollander had showed the psychic the photograph of a man whom he suspected of fraudulent dealings in coffee. "Mr. Croiset did not know the man, nor did the official volunteer any information" we are assured. The clairvoyant made a number of statements about the suspect, almost all of which were correct. Remarkably, Croiset told Den Hollander about some details that were at that time unknown to the police but which were subsequently verified. An example: "The coffee has not disappeared across smugglers' trails, but normally through the custom's barriers," Croiset had said. Tenhaeff quotes Den Hollander's comments: "Unknown during consultation. Afterwards, it was discovered that part of the coffee went over the border through the barriers. The coffee had been hidden in a limousine."

From a number of details in Tenhaeff's report, the case can be positively identified as the smuggling affair in which a Mr. G. Hasperhoven, director of a coffee-roasting factory in Enschede, was involved. Prior to the consultation, the case had received nationwide publicity. The name of the prime suspect (certain details in Tomorrow-account strongly suggest that this was the man whose photograph had served as the "inductor") had been mentioned in the local press. Croiset, who at that time lived in Enschede, must have been aware of it. Local gossip (Enschede is a border town and was a center of smuggling in the early fifties) may very well have provided him with bits of information the authorities were not yet aware of. Even if we assume that Croiset had never seen the man on the photograph

(How could Tenhaeff have known this, incidentally?), we must admit that he could safely have guessed that the consultation was somehow related to the smuggling affair. After all, his client was an official of the Customs' Department! Conjectures apart, there remains something unsatisfactory in the evidence. Some essential questions are not answered. Who took the initiative in the consultation? Were Croiset's statements recorded immediately and in full? Were there other witnesses? Pollack, who describes the case on pp 90-91 of his biography, insists that on April 11, Den Hollander wrote to Tenhaeff "thanking him for the invaluable help of Gerard Croiset in cracking this case; in disclosing exactly how the smuggling ring operated; and for furnishing key information that the customs department didn't have." Tenhaeff, however, does not mention a letter. He states that Den Hollander told him about the case, which suggests an oral report. The Dutch version of Den Hollander's comments, published in Beschouwingen²⁰ bears the unmistakable marks of Tenhaeff's own solemn and verbose style. If Mr. Den Hollander is still alive, I have been unable to locate him. I would have liked to ask him if he had indeed told Tenhaeff that at the time of the consultation (Nov. 10, 1952) the authorities did not yet know that the coffee had been smuggled not across smugglers' trails, but normally through the customs' barriers, hidden in a limousine.

I seem to notice a discrepancy with the fact that already on Monday, October 27, the Enschede newspaper Tubantia had mentioned the Limousine and that, on November 5, the same paper had reported that the customs department had staged a reconstruction of the way the coffee had been smuggled. That report was accompanied by a photograph on which both the car and the customs barriers can be seen clearly.

THE WOERDEN CASE

On the final page of his Tomorrow paper, Tenhaeff relates Croiset's involvement in the solution of a spectacular crime that had occurred less than one year before the Utrecht lecture was read. This account deserves to be quoted in full.

"In October 1952, a sensational attempt was made to murder a policeman on patrol in the municipality of W. The day after the news had been published in the papers, Mr. Croiset informed me that while reading the news, the image of a well-known shop in Utrecht had forced itself upon him. In this shop stage properties are sold and hired. A suit of ancient armor has stood for many years in one of the windows. The image of this suit of armor had forced itself upon him very distinctly. Besides, Mr. Croiset had the "impression" that the guilty man had formerly worn a uniform.

"Mr. Croiset suspected, on the basis of these "impressions," that the criminal must be somewhere in the vicinity of this shop.

"Ten days after this telephone conversation, I was in the law court in Utrecht with Croiset. On the table was a parcel of objects belonging to the policeman who had been

attacked. While it was still unopened, Mr. Croiset was able to inform us that there was a revolver in the parcel (which turned out to be correct). He, Mr. C., then began to communicate "impressions" about the criminal. He was able to say, for instance, that this man liked fishing, and kept a little boat. The image of an iron ell-pot also forced itself upon him. He exclaimed:

"Now I understand the image of the armor. Such armor is made by a metal worker and that ell-pot is also made by a metal worker. This man (the criminal) is acquainted with a metal worker who has made it. It may also be that the man himself is a metal worker."

"After Mr. Croiset had communicated to those present further 'impressions' in connection with this case, the investigating judge told us that a metal worker - who possessed a small boat and an ell-pot and formerly wore a uniform - had been arrested on suspicion.

"When we know that Mr. Croiset's parents were connected with the stage and that his brother Max, like his father, is a well-known reciter, we can understand why the image of the armor in the window forced itself upon him when he heard about the attempt on the policeman's life. Apparently Mr. Croiset already knew unconsciously, thanks to his psychic gifts, that a metal worker was in some way involved in the attack. Because of his interest in the stage, partly connected with youthful experience, the word metal worker was associated by him with the familiar suit of armor."

In 1957 Tenhaeff related the case again in his Dutch book Beschouwingen over het gebruik van paragnosten and again one year later in his German "Ueber die Anwendung paranormaler Fahigkeiten" and, finally, in 1960, in his English "The Employment of Paragnosts for Police Purposes." These three versions being practically identical I will restrict myself to referring to the English source.²¹

There are interesting discrepancies with the 1953 Tomorrow version. There, Tenhaeff claims that Croiset had "seen" the uniform before any suspect had been arrested. In the 1960 article, however, we are told that this hit was scored ten days later - after the arrest had been made. In 1953, Tenhaeff creates the impression that the metal worker "seen" by Croiset had actually been involved in the assassination attempt. Surprisingly, in the 1960 account we learn that this was not the case.

"For the sake of completeness," we read, "it should be mentioned that the arrested tin-smith had been suspected wrongly; he was set free soon after the consultation. The case can thus serve as an example of a consultation which failed from the police angle (but succeeded from the parapsychological angle). It is also of interest that Alpha (Croiset's code-name) 'saw' the breastplate at a time when the sheet metal worker had not yet been arrested. One may surmise that the paragnost had obtained an impression of a future mistake on the part of the police."

The 1960 account gives some additional details. At the consultation in the room of the law-court, Croiset had not only "seen" the revolver, but had also received an impression of "spokes." "The presiding judge, who was present at the inquiry, said that the picture was correct. When the policeman was shot down, he not only dropped his revolver but also his bicycle. One of the wheels of the bicycle came to lie on top of the revolver." The name of the municipality is now mentioned in full: Woerden, in the province of South Holland, not far from Utrecht.

What is implicitly denied in the 1953 account is admitted in the 1960 version: Croiset had utterly failed in his attempt to help the police solve a major crime. Yet Tenhaeff insists that the case was highly successful from the point of view of the psychical researcher. The psychic had picked the wrong man, but he had paranormally seen and described a suspect in specific detail. He had mentioned this man's profession, his fondness of fishing and the fact that he had worn a uniform.

Striking as this may seem, it will hardly do as evidence for ESP. For Croiset had "seen" the metal worker only after the latter had been arrested. The Professor does not tell us what precautions had been taken to keep the clairvoyant from learning of this arrest by normal means. Prior to the arrest, Croiset had got no further than making vague statements about a Utrecht shop and a suit of armor displayed in one of the windows. Tenhaeff is deeply impressed with the armor, which he invites us to believe, was a striking hit somewhat distorted by the unconscious processes inside Croiset's brain. But, of course, given this freedom to indulge in post factum "interpretations," any psychic reading can be made to fit any conceivable suspect. The "impressions" would have been at least as apposite if, for example, the suspect had been a soldier or someone somehow connected with the stage, if he had lived near the street mentioned by Croiset, or near the statue of a man wearing a mediaeval suit of armor. No doubt Tenhaeff would have hailed a remarkable hit if the suspect's name had been "Smit" ("smith") or "De Ridder" ("knight"), both very common names in Holland. And this by no means exhausts the supply of possible "matches."

So even if we accept Tenhaeff's 1960 version of the facts the case is unconvincing. However, worse is to come. It will be recalled that, in his Tomorrow report, Tenhaeff spoke of a "sensational" crime. This caused me to wonder whether it might be worth the trouble to search the newspaper files for information relevant to the present inquiry. My visit to the archives of De Telegraaf proved highly rewarding.

The assassination attempt, so I learned, took place not in October but in November 1952, in the early morning of Friday 14. The victim, policeman Van Eck of Woerden, died before he arrived at the hospital. That same morning, De Telegraaf carried the story prominently. That report mentions the fact that Mr. Van Eck was riding a bicycle when he was shot. To the critical reader, this may suggest a possible non-paranormal source for Croiset's "impression" of "spokes," received ten days later. (The "vision" of the revolver is hardly more striking. Apart from the possibility that the shape of the parcel may have inspired Mr. Croiset, I must point to the fact that the policeman had been on his way to a burglary alarm and so had been armed as a matter of course.)

In all his published accounts, Tenhaeff states explicitly that Croiset received his "impressions" of a "metal worker" who was fond of fishing at a seance that took place ten days after he had phoned his mentor. The phone call had been "on the day after the news had been published in the newspapers," so the consultation in the court room has to be dated Tuesday, November 25.

De Telegraaf confirms that a metal worker was arrested. However, this metal worker was not the one who had "formerly worn a uniform." As it happened, there were two suspects. One of these was the 36 year old metal worker K.V.; the other one 31 year old D. van H., a civil servant who had formerly been a member of the Woerden police. The two men, who were both said to be poachers, had gone out together on the night of the murder. Both later proved to be entirely innocent.

The crucial fact is that the arrest of the two suspects took place on Wednesday, November 19, and was reported in the national daily newspapers on the 20th. On that day, De Telegraaf published the initials of the two men, mentioned their professions and former professions and did not neglect to remark on their fondness of fishing!

The details Croiset paranormally perceived during the consultation in the court room had all been published in the papers five days previously. By entirely suppressing this essential bit of information, Tenhaeff was able to present this non-event as a convincing example of extrasensory perception.

CONCLUSION

A critical and detailed examination of four cases of psychic detection has led to the discovery of glaring flaws in the published evidence. It is of utmost importance to note that these were prize cases involving one of the best known occult sleuths in history and reported either directly by or under the supervision of "a noted world figure in parapsychology." As the motto of his book, Mr. Pollack had chosen Charles Richet's celebrated dictum: "I will not say that it is possible. I only say that it is true." As far as the four prize cases analysed in this article are concerned I prefer to say; "Je ne dirai pas que cela est impossible. Je dis seulement que ce n'est pas vrai."

NOTES:

- ¹ Hoebens, P.H., "Gerard Croiset: Investigation of the Mozart of 'Psychic Sleuths' - Part I," The Skeptical Inquirer, 6, 1 (Fall 1981), 17-28.
- ² Zorab, G., "Review of Jack Harrison Pollack's Croiset the Clairvoyant," Journal of the Society for Psychical Research, 43 (1965), 209-212.
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- 14 Pelz, C., "Herr Croiset, Sie können nicht hellsehen," Kosmos, 1959/60.
- 15 At the end of the relevant chapter, Pollack mentions "the lower left side." Presumably this was a typing error.
- 16 Pelz writes at great length on this striped awning, both in his Kosmos article and in an article submitted for publication in Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie on 3-28-1958 and rejected 4-10-1958.
- 17 Tenhaeff's own account of this case, in his article "Ueber die Anwendung paranormaler Fähigkeiten" (Zeitschrift für P., 2, 1, 1958, 13-14) is comparatively accurate. At least he mentions the fact that the body was found far from the factory.
- 18 Tenhaeff, W.H.C., "Aid to the Police," Tomorrow, 2, 1 (Autumn 1953), 10-18. This is the second part of a double article by Bender and Tenhaeff. When, in his 1961 book Hellseher Scharlatane Demagogen (Munich/Basle: Ernst Reinhardt Verlag), Wilhelm Gibisch criticized Bender for some remarks made in the first part of the article, the German parapsychologist Anton Neuhäusler claimed (Zeitschrift für P., 7, 2/3, 1964, pp. 102 & 113) that the Tomorrow-article had not been written by Bender and Tenhaeff. According to Neuhäusler, it was a condensation of the papers read at the Utrecht conference, made by a journalist and without the authors' fiat. Tomorrow clearly mentions Bender and Tenhaeff as the authors. To avoid any possible misunderstanding, I consulted the stencilled Proceedings of the 1953 conference, a copy of which is kept at the Royal Library in the Hague. As far as the two cases dealt with in the previous case are concerned, the versions in Tomorrow and in the Proceedings are identical.
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- 22 Similar examples of psychoanalytical acrobatics are found in Tenhaeff's often hilarious reports on the celebrated "chair tests."

ZS Dialogue

INTRODUCTION TO "RESEARCH ON THE MARS EFFECT"

MARCELLO TRUZZI

The following article by Mr. Curry was received by ZS in July of 1981. While being prepared for publication and out for commentaries, the article "STARBABY" by astronomer Dennis Rawlins (a former Fellow and Executive Council member of the Committee for the Scientific Investigation of Claims of the Paranormal -- or CSICOP) appeared in the October issue of Fate magazine. Rawlins alleged that CSICOP had not only demonstrated gross incompetence but had engaged in what he termed a "cover up" and "censorship" in its handling of the tests it sponsored on psychologist Michel Gauquelin's "neoastrological" Mars Effect claim. Rawlins alleged that his own ejection from CSICOP was the result of his attempts to get CSICOP leadership to publicly confess their errors. This defense of Gauquelin's claim was particularly surprising since Rawlins was a vigorous opponent of the Mars Effect and a strong critic of Gauquelin.

Since then, there has been much controversy surrounding Rawlins' charges (even resulting in several resignations within CSICOP). This has unfortunately resulted in polarization into black-white issues when they are actually quite gray. Issues of science seem to have given way to issues of personalities. For the record, let me here state that I (and I speak only for myself and not my other editors) do not fully agree with all of Rawlins' many charges; but I must also indicate that I do not find the responses from the CSICOP leadership either adequate or credible --at least so far. But whatever my personal opinions might be, I have urged and continue to urge CSICOP to make full public reply to Rawlins. I urge Fate to publish any official reply by CSICOP, and if no such reply is published there or in The Skeptical Inquirer, I offer space in ZS for such a reply. Thus far, CSICOP has limited its reply to a short statement in The Skeptical Inquirer and two papers ("Status of the 'Mars Effect'" by Profs. Abell, Kurtz and Zelen, and "Crybaby" by Philip J. Klass) which can be purchased from CSICOP. "Crybaby" purports to respond directly to Rawlins but thus far has not received the official endorsement of the Executive Council. (Fate's editor refused publication of "Crybaby" giving in part the reason that it lacked such endorsement.) Whatever CSICOP's reply, since Rawlins charges have been made publicly, the reply should also be public and available for possible rejoinder.

What follows is an independent critique by Patrick Curry, an historian of science with special interest in astrology. Readers of Rawlins' article will find Curry's analysis comparatively dispassionate and more interested in issues of method than those of motives. Curry's charges are therefore more serious. Mr. Curry sought information from all the principals in this controversy, both within and outside CSCOP. In addition, I sent all key CSICOP participants advance copies of his article (about five months ago) with an invitation to comment/respond for publication in this same issue of ZS. Though no member of the CSICOP Executive Council has yet responded for publication in ZS, it is hoped that such responses may come for publication in future issues of ZS. It is also to be hoped that other CSICOP Fellows, those not invited in advance, will want to contribute their comments to this ongoing ZS Dialogue.



RESEARCH ON THE MARS EFFECT

PATRICK CURRY

PART I

Introduction

In modern disputes over the scientific status of astrology, the centerpiece of discussion has always -- rightly or wrongly -- been the work of Michel and Francoise Gauquelin. Recently, the American Committee for the Scientific Investigation of Claims of the Paranormal (or CSICOP) carried out two tests of one of Gauquelin's findings, namely, a statistically significant link between the position of Mars at birth and professional success in sports. Known for brevity as "the Mars effect," the original analysis of 1,553 sports champions was published in Gauquelin (1980).

The CSICOP's two tests were: (1) a test of Gauquelin's theoretical calculations for the expected frequencies of Mars, called "the Zelen test" and published in The Humanist (Nov.-Dec., 1977); and (2) an attempted replication of Gauquelin's results, using a fresh sample of American athletes. This was published in The Skeptical Inquirer (Winter, 1979-80), with a "Follow-up" in the issue of Summer, 1980. Confusingly, both sets of papers were accompanied by analyses by M. and F. Gauquelin which came to diametrically opposite conclusions concerning the outcomes. The reader was therefore left in some doubt as to the final outcome (if any).

My intention in this paper is therefore two-fold: to assess the scientific status of (1) the CSICOP's work, and (2) the Mars effect itself.

Readers are likely to be relatively familiar with only the above-mentioned articles. For this reason, I decided on a thorough analysis of their contents, rather than a simply chronological narrative. My analysis will rely in part on unpublished memoranda and correspondence, and I would here like to thank the three parties involved -- M. Gauquelin, the CSICOP, and its ex-member Dennis Rawlins -- for their cooperation.

I should add that I am less interested in possible psychological bias underlying claims (which is probably unavoidable, to some extent) than in the objective and independently assessable evidence for and against such claims. The former has more to do with sociology of science than scientific discovery. Bias is important in terms of motivation and social context, but (in my view) it only supplants the canons of scientific rationality, broadly speaking, when the latter have seriously broken down.

Background: Gauquelin

The Gauquelins' work has been going on over the last twenty years. Their data, methodology and results have all been published by their Laboratoire d'Etude des Relations entre Rythmes Cosmiques et Psychophysiologiques in Paris. The results include disconfirmation of several standard astrological concepts

(e.g., the "houses" and "signs," including the ubiquitous "sun sign").

More controversially, they point to highly significant correlations between the positions of certain planets (the Moon, Venus, Mars, Jupiter and Saturn) in the diurnal (24 hour) circle at birth, and three empirical phenomena on Earth: (1) a high degree of professional success, (2) psychological temperament, as measured on appropriate tests and measures, and (3) a tendency for children to be born at such times as to share or "inherit" the same planetary placements as their parents. The total sample, including control groups, now well exceeds 100,000.

These results are not without astrological significance. The relevant sectors of the sky are (roughly speaking) those of "rising" and "upper culmination." These are not the sectors predicted by post-Ptolemaic astrology, but would not have surprised the original (as far as we know) Babylonian astrologers. More importantly, the planetary correlations are those specifically predicted by traditional theory -- e.g., Mars and aggression, Saturn and introversion, etc. (see Gauquelin, F., 1980a; Startup, 1981; Gauquelin M. & F., & Eysenck, S.B.G., 1979) Furthermore, the "natures" of the planets are one of the few parts of astrological theory that are uncontroversial among astrologers themselves.

Clearly, the Gauquelins' claim amounts to a highly controversial one for scientists -- that is, in terms of the present body of scientific knowledge. Psychologically speaking, this implies unusual difficulty in giving the claim a fair and objective evaluation. At the same time, however, the chance of a radical "new" finding demands just such an evaluation. Especially important, perhaps, is adequate replication by independent observers.

Comité Para

The first replication-attempt was by the Belgian Committee for Scientific Investigation of Alleged Paranormal Phenomena (or Comité Para), in 1968. The Comité Para chose to test one of Gauquelin's highest claimed correlations, the Mars effect. According to the Gauquelins, Mars appears in critical sectors one ("rising") and four ("upper culmination") in 22% of the births of 1,553 sports champions, compared with 17% for non-champions. This is significant at roughly odds of 5 million to 1 (against chance).

The Comité Para accordingly collected and analyzed a new sample of 535 sports champions. They found that "The distribution of the actual frequencies of Mars is far from uniform: they display the same general pattern found by M.M. Gauquelin with samples of other sports champions....The Comité therefore gives its agreement on this point with the results of M.M. Gauquelin" (Dommangelet, 1976).

However, the Comité declined to cite this result as support for Gauquelin, citing (unspecified) demographical errors in the calculation of theoretical, or expected, frequencies. (Further control tests of their own on this problem remain unpublished.)

There is one further point about the Comité Para results. According to Kurtz (e.g., 1981e), who is here quoting Rawlins, those results disconfirm Gauquelin's hypothesis because "although sector one has a higher frequency than expected, sectors 9 and 10 were higher than [the other predicted sector,

number] 4, and 8 was considerable higher than expectation." (You will recall from above that this was not the Comité Para's conclusion.) However, this is not as serious for Gauquelin as it may sound. His claimed planetary patterns have always included lesser "peaks" in sectors seven and ten (which point is not ad hoc, having been made consistently since at least his 1960 book). Estimation of how anomalous the peaks in sectors eight and nine are would have to take into consideration the sample size, which is adequate (at 535) but considerably smaller than Gauquelin's whole sample.

The Zelen Test

This set of published documents consists of an introduction, "The Mars Effect and the Zelen Test," by Paul Kurtz; "The Zelen Test of the Mars Effect," by M. & F. Gauquelin; and "Is There a Mars Effect?" by Professors Kurtz, Marvin Zelen and George Abel (henceforth for convenience designated KZ&A).

The Zelen test is so-called because it was first proposed by Zelen as a "Challenge" to the Gauquelins, in the Jan.-Feb., 1976 issue of The Humanist. Its purpose was to test Gauquelin's theoretical figure of 17% for his control-group of non-champions. In other words, it would test the Comité Para's reservations regarding that figure.

The procedure agreed-upon was relatively simple, in principle. It involved collecting a new control-group of non-champions born in close temporal and spatial proximity to a representative sample of 303 champions. (Gauquelin indicates the statistical reasons why "300 cases of champions, and many more cases of non-champions, appear to be the minimal conditions for reaching the level of significance" (1977b, p.33)).

If this new group showed a significant Mars effect or incidence of 22%, it would deflate Gauquelin's claim of a special significance for Mars and the births of champions. On the other hand, if it showed an incidence of 17%, this would confirm the correctness of his figure for the population -- thus supporting the existence of a Mars effect.

The data -- 16,756 non-champions -- were collected by the Gauquelins (with some assistance for Belgium) and calculated according to Zelen's procedure. The results showed a difference between the (303) champions and the larger sample which was significant at .03 (according to Gauquelin) or .04 (according to KZ&A). (We shall discover the reason for the divergence below.) Put another way, the Mars effect appeared only with the sports champions, and not in the general population born close-by in time and place.

In their report, KZ&A raise two objections to Gauquelin's verdict of vindication. These are: (1) that the overall significance of .04 is due to a single key-sector birth. "If there was one less champion birth in the key-sector (62 rather than 63), the difference would be associated with a P-value of 0.07" (KZ&A, 1977, p. 37). Secondly (2), "...there is a statistical difference in the proportion of key sector births among champions versus non-champions for Paris..." ($n=42$), but not for the rest of the sample of 303 -- i.e., France minus Paris ($n = 196$) and Belgium ($n = 65$) (also from p.37).

Further to the second point, KZ&A question Gauquelin's concentration on the urban "chefs-lieux" for the champions and the control group. (He adopted this strategy for methodological reasons described in his 1977b, p.31.) If there is a geographical element in the Mars effect favouring cities (or just Paris), the latter may thereby make the kind of disproportionate contribution

that KZ&A claim it does.

The Gauquelins defended the objectivity of their sample on these grounds: champions in the list (and therefore their control "companions") are born in many different and well-scattered chefs-lieux, and their dates span 1870 to 1945. Furthermore, the 303 champions show a Mars effect of 22%, in line with both the whole original sample of champions ($n = 2,088$), and that sample minus the 303.

The Zelen Test: Assessment

Now let us examine the points made by KZ&A more closely. With respect to the figure of .04 due to a single key-sector placing, it is important to realize that they dropped female sports champions from consideration. KZ&A justify this with the curious two-fold rationale that "It is clear that nearly all of our sports champions tend to be male. Further, assuming the existence of a Mars effect, a male born in a key sector is more likely to be a future sports champion than a female born in a key sector" (p. 37). Be that as it may, the reader is not informed that of the nine female champions eliminated, three had Mars in key sectors. KZ&A's criticism therefore is less than entirely accurate, or given the vagueness of its rationale and discussion -- fair.

Taking up the second criticism -- an anomalous Paris sample -- it is obviously questionable for KZ&A to draw their main conclusion from a post hoc separation of the data into sub-samples. It was left to Eric Tarkington, in an astrological publication (*Phenomena* 2.2, 1978) to point out that removing the Paris portion breaks the data into two parts: "one very small and [therefore] very unreliable; and one consistent with a Mars effect, but a little too small to achieve significance" (p.19). (I would remind the reader of the "minimal" figure of 300, mentioned on p.3 above.) Tarkington also demonstrated that the Paris sub-sample is not inconsistent with the rest of the sample (as shown by a hypothetical sampling distribution of 40-champion samples, with its mean and standard deviation from the complete sample). Further, breaking the sample into three parts -- Paris, the rest of France, and Belgium -- only compounds the error, without altering the fact that the three do not statistically differ; that is, all are within the confidence limits (at the level of .05).

These points, at least in essence, were available to the CSICOP before publication. Professor Elizabeth Scott (a member of the "Gauquelin sub-committee" of CSICOP), had registered dismay at the MS stage over breaking up the sample, "because I feel strongly that the discussion may be misleading" (Rawlins, 1980b).

Still earlier, Rawlins had circulated a Memorandum of March 29, 1977, to several members of CSICOP. In it, he analyzed the only concrete suggestion to date of a possible demographic-astronomical cause of a spurious Mars effect. That is, the tendency for Mars to be near the sun, in geocentric longitude; and the peak in births near sunrise. He concluded that "Gauquelin has made fair allowance for the effect under investigation."

Rawlins also noticed that the Zelen test presumes a "clean sample" on Gauquelin's part (something that CSICOP would later question). As he later put it, "if the Mars effect is due to G.'s pre-1968 sampling being awry, then the (in) famous non-champions 'Challenge' would inevitably come out in Gauquelin's favor"(1980a). According to Rawlins, he communicated these points to Kurtz (Jan. 23, 1976), Zelen (March 8, 1976) and Abell (Dec. 6, 1975).

In a letter to Kurtz on April 29, 1977, Abell admitted that the 17% figure had "in a sense" been "vindicated," and he described the verdict of the test as "significant." Abell also noted that the 22% figure applied equally to the 303 and the original 2,088 champions.

Despite all this, the Zelen test report appeared in its present form in The Humanist of Nov.-Dec., 1977. Following publication, the criticisms of Tarkington appeared which we have noted. And independently, Rawlins sent to Kurtz (on April 6, 1978) a letter briefly showing that there are no significant geographic variations within the Zelen test sample. Rawlins also reminded Kurtz of his (Rawlins') 1977 Sun/Mars memorandum.

Apparently obtaining no satisfaction on these points, Rawlins submitted his Report on the U.S. test (Sept. 18, 1978), asking for a published correction of CSICOP errors and insisting, this time, on "No fake unanimity." This was revised and resubmitted on Oct. 18, 1978, (and extensively re-edited by The Skeptical Inquirer). On Nov. 6, 1979, he requested competent independent refereeing of the Zelen test results. However, the only comment to reach the public was a laudatory letter from L. Jerome congratulating CSICOP on its confirmation of his 1975 critique of Gauquelin. (This critique had been demonstrated to be erroneous in a memorandum by Rawlins in 1978b.) And on Dec. 12, 1979, Rawlins was unanimously "not renominated" by the CSICOP Executive Council. (The official reason(s) were never given, in writing or otherwise.)

In the meantime, The Skeptical Inquirer had published its "Four-Part Report..." on the U.S. replication (Winter, 1979-80). Discussion therein by KZ&A of the Zelen test reiterates their former charge (reviewed above): an anomalous Paris sample. They defend dropping female champions -- still without discussion of the consequences -- because "surely women have not had the same opportunities men have had to pursue sports" (p.21). (Is there any independent evidence to this effect?)

In sum, what are we to make of the Zelen test and its handling by CSICOP? A charitable interpretation is made difficult by the facts reviewed above. It seems clear that KZ&A ignored repeated 'internal' warnings prior to publication of their misleading interpretations -- particularly from Rawlins, the only planetary-motion specialist involved. The errors went uncorrected in The Humanist and were even repeated in The Skeptical Inquirer. (Perhaps I should add that Rawlins was dismissed from that journal's Editorial Board by its editor in the spring of 1980).

Apart from the specific points discussed above, after the Gauquelins had collected a new sample of 16,756 non-champions and after the results of the Zelen test were known, KZ&A questioned Gauquelin's original sample of 303 champions. In effect, what had been a test of non-champions was turned into a test of champions. The original formulation of the Zelen test had been quite clear on this -- it was a test in order to establish whether the "chance" level was 17% (as Gauquelin claimed) or 22% (as the Comité Para hinted, and CSICOP seems to have believed, in spite of Rawlins' warnings to the contrary).

It is true that the placements of Mars in the sample of 303 champions are not strongly significant. However, no sample of that size will produce strong significance if one is testing a difference between 17% and 22%.

CSICOP's final interpretation looks still more questionable in the light of their own original description of the Zelen test as "...an objective way for unambiguous corroboration or disconfirmation...[Thus we may] settle the question" (Zelen, 1976). Abell's comment was that it "appears to be a definitive test" (1976b). (He went on to say that the Zelen test "will be refereed by a disinterested and competent committee of scientists." It is unfortunate that nothing ever came of this.)

The outcome of the Zelen test does not unequivocally establish Gauquelin's case -- leaving aside the tendentious matter of exactly what would do so. However, the outcome does unequivocally support his case, as Rawlins was permitted to observe in the only public statement so far to that effect -- a footnote in his last and heavily-edited contribution to The Skeptical Inquirer (Winter 1979-80, p.30).

On May 3, 1980, Abell wrote Gauquelin to say that "the excess of athletes in that sample [of 303] born with Mars in sectors one and four over the numbers expected by chance is not due to any approximations you may have used in handling the astronomical data to calculate the expected distributions." Such clarity in private leaves little excuse for the public conclusion of KZ&A in 1977 (p. 38) -- a psychological truism about high and low prior beliefs, etc., of no scientific consequence.

PART II

Introduction

In what follows, I will present, in order: (1) a synopsis of the U.S. test of the Mars effect (KZ&A and Rawlins' interpretation, then Gauquelin's; (2) an assessment of the U.S. test, drawing partly on unpublished material; (3) a synopsis of Gauquelin's new European replication; (4) an assessment; (5) other developments since the published documents appeared; and (6) overall conclusions and discussion.

The relevant published documents are: "Four-Part Report on Claimed 'Mars Effect'" (The Skeptical Inquirer, Winter 1979-80, pp.19-63), consisting of "Results of the U.S. Test of the 'Mars Effect' Are Negative," by Kurtz, Zelen and Abell; "Report on the U.S. Test of the Gauquelins' 'Mars Effect'," by Rawlins; "Star U.S. Sportsmen Display the Mars Effect," by M. & F. Gauquelin; and "Response to the Gauquelins," by KZ&A; and the "Follow-up (SI, Summer 1980, pp. 56-68), consisting of "The 'Mars Effect': A Response from M. Gauquelin," and "The Contradictions in Gauquelin's Research: Rejoinder by Kurtz, Zelen and Abell." (In order to avoid a tedious repetition of each article, my synopses will only cover principal points.)

U.S. Test Synopsis/KZ&A and Rawlins

For a "representative sample of U.S. sports champions," KZ&A choose all those listed in the Lincoln Library of Sports Champions (Frontier Press, 1974) (340 names), plus 218 names from Who's Who in Football (Arlington House, 1974) and 47 from Who's Who in Basketball (Arlington House, 1973). (According to the report, "the actual selection was made by two neutral researchers, Frank Dolce and Germaine Harnden.") Requests for the birth data of this sample were then sent to state registry offices, which resulted in data for 128 champions. Of these, 25 had Mars in critical sectors -- i.e., 19.5%, which does not significantly differ from the expected 17%.

Given the smallness of the sample, it was decided to expand it. A second canvass culled "remaining champions" from W.W. in Football (330) and W.W. in Basketball (145), and 111 names from Who's Who in Track & Field (Arlington, 1973), and 92 from Who's Who in Boxing (Arlington, 1974). For this sample, replies were obtained for 197, of whom 24 (12%, also non-significant) display the Mars effect.

A third sample of "athletes listed in the directories but whose names had been omitted" (p.22) resulted in data for an additional 83, with a Mars effect of 6 (7% -- actually significantly low, at the level of .02).

Rawlins' brief contribution elaborates the statistical implications of the overall result -- a Mars effect of 55 out of 408, or 13.5%. This figure is "distinctly (but not significantly) below chance expectation." Rawlins reiterates KZ&A's conclusion that "the analysis of American sports champions shows no evidence for the Mars effect" (p.25).

U.S. Test Synopsis/Gauquelin

Contrary to the conclusion just stated, the Gauquelins' position is: "The data on the 'star U.S. sportsmen' (Rawlins dixit) strongly display the Mars effect." They first remind the reader of their two long-standing conditions for observation of the Mars effect: (1) the sports figures' births should be natural ones, i.e., unaffected by medical intervention; and (2) only "the greatest names" should be chosen, as merely moderately successful athletes will not show the effect. Since only 10 percent of KZ&A's sample were born after 1950, the Gauquelins state that its lack of a Mars effect does not result from non-observance of this condition. However -- and this is the essence of their objection -- the second condition "is not respected; there are few 'all-time great' names in the sample" (p.32). For example, The World Almanac and Book of Facts (1978) lists 93 members of the Pro-Football Hall of Fame, "but only 5 of them appear in the K-Z-A sample," also 163 champions in the National Baseball Hall of Fame, of whom KZ&A only include 3.

The Gauquelins then proceed with a post-hoc analysis of the data in order to demonstrate that it does, in fact, support their hypothesis. The principal subsets involved are KZ&A's first sample, and a partly new sample.

Regarding the first sample, the Gauquelins note that the Mars effect is in the correct direction for their argument, that it increases to 20.3% if post-1950 champions are removed, and that it displays the Mars effect significantly more often than the whole sample of 408. Strongly questioning the second and third "canvasses," they point out that: (1) KZ&A didn't try in any way to obtain the data of the (436) champions remaining from the first selection; instead, they wrote to states who had answered requesting data for other sportsmen; (2) for this purpose, KZ&A drew on W.W. in Football and W.W. in Basketball for 216 out of 280 names (second plus third selections); having already been used, these volumes resulted in a diluted sample (as well as some inconsistencies, such as the inclusion of some coaches, etc.). The authors point out that KZ&A's first sample plus the names from W.W. in Track & Field and W.W. in Boxing (=192) shows a Mars effect of 20%, while the remaining 216 members of the subsequent selections show one of 8%.

Finally, the Gauquelins offer a sample of "really prominent U.S. athletes," derived from the Lincoln Library of Sports Champions (73 names used by KZ&A), plus the World Almanac... (1978) (31 "Notable Sports Personalities" and 20 U.S.

Olympic Champions, drawn by KZ&A). Omitting duplicates, this sample of 88 shows a Mars effect of 19, i.e., 21.6%. (Further omitting 7 athletes born after 1950, it rises to 23.5%.) Comparison is invited with the Mars effect in the whole sample of 13.5%. (It is also mentioned that "Those who achieve the highest Olympic honors seem also to display the highest Mars effect" (p.37) -- e.g. of the 20 mentioned above, 7 or 35%.) "The conclusion is clear: the Mars effect is linked to the degree of celebrity and achievement of the athletes" (p.38).

Responses from KZ&A, Gauquelin, & Rejoinder by KZ&A (U.S. Test)

In their response, KZ&A contest Gauquelin's second point (above), concerning non-observance of the "greatness" stipulation. They say that "in our original conference with M. Gauquelin, it was agreed that we would select football and basketball stars from the Who's Who directories and also use the Lincoln Library... as the basis of our sample (p.45). They add that in the second and third samples there are "almost as many All-Stars and All-Pro's in football and basketball as in the first selection" (p.47). Furthermore, total All-Stars from neither sport show the Mars effect. KZ&A point out that all the sub-samples that Gauquelin cites in his favour -- e.g., 73 champions from the Lincoln Library (with a Mars effect of 19.2%), 15 boxers (20%) and 51 track & field stars (19.6%) used, etc. -- show results "within the range of chance." (Deletion of 10 coaches would evidently make "no significant difference.") They go on to offer their own alternative to Gauquelin's 88 "superstars." This particular post hoc sample consists of athletes from: the Lincoln Library (73 names), plus Olympic gold medallists (73) and World Almanac "Notable Sports Personality" (1) not already in the L. Library; additional "renowned champions" from the World and Hammond Almanacs (25), and All-Star and All-Pro player (65).* This total of 181 -- a selection, they contend, as distinguished as Gauquelin's -- has a Mars effect of only 16.6%, or almost precisely chance expectation. Finally, KZ&A state that "In view of the Gauquelins' new claims regarding sample selection, it is perhaps necessary to review their original study of 2,088 European sports champions" (p.53-4). (Objections regarding sectors emphasized will be discussed below.)

In his reply, Gauquelin repeats his assertion that the most outstanding athletes show the Mars effect. He says that if they had been asked beforehand, he and F. Gauquelin "certainly would have agreed with the choice of the Lincoln Library... but we certainly would not have agreed with the use of W.W. in Football, and W.W. in Basketball without making any prior selection among the thousands of players included in these books" (1980, p. 59). Regarding KZ&A's use of the Hammond Atlas, he points out that they omitted 29 outstanding champions listed therein; adding these to the 53 used by KZ&A results in 82, with a Mars effect of 19 -- i.e., 23.2%. Other selections of top athletes out of KZ&A's data show similar results, e.g., 32 who are listed in F. Litsky's Superstars (1975), show a Mars effect of 10, or 31.3%. Gauquelin contrasts these figures with KZ&A's overall finding of 13.5%.

Finally, the "Rejoinder" by KZ&A states: "In a meeting between Michel Gauquelin and KZ and A in July 1977, before we began our research, Gauquelin agreed upon the use of the Lincoln Library..., W.W. in Basketball, and W.W. in Football, although he now objects to the use of the latter two volumes" (1980, p.62). They reassert the comparability of their "181" with Gauquelin's "88," as well as an 'anomalous sector' claim (to be discussed below). Finally, they again attack Gauquelin's original sample of 2,088, as employing inconsistent criteria -- both internally and compared to his apparently stricter American samples.

*Minus overlaps.

U.S. Test/Assessment

I now turn with some relief from listing this welter of conflicting claims-and-charges to analyzing it. Perhaps the most important point to be made is this --the whole mess (if I may be blunt) clearly could have been avoided by the simple means of protocols, agreed upon in advance and in writing by all the parties concerned. As we have seen above, KZ&A maintain that prior agreement was obtained; elsewhere (1980, p.67) they write, "we decided to do an independent U.S. study -- so that the data could be checked step-by-step by both the Gauquelins and the Committee." And as we have also seen, Gauquelin denies that such consultation, let alone agreement, ever occurred. Since the lack of any written agreement has left us in this position, we must ask -- what does what evidence there is have to tell us?

Given the voluminous KZ&A-Gauquelin correspondence, it is *prima facie* very strange -- if an agreement on sources to be used ever existed -- that none of it from 1977-78 (that I have seen) makes any reference to it. The most telling letter is from Abell to Gauquelin, dated (note:) Feb. 21, 1978. The second paragraph reads: "At this time I don't have anything to report to you about the Mars effect, but I do understand that some people are trying to obtain data on U.S. athletes. I presume this is with your knowledge and cooperation and would be most interested to hear from you how it is turning out."

In other correspondence, Abell says regarding the 1977 meeting that "Unfortunately, there was at best only a verbal agreement (and quite honestly I do not recall what, if anything, was firmly agreed to at that meeting)..." (1981d). He further recalls that "I did not actually see any of the data or results until Paul [Kurtz] sent them to me asking if I could verify Dennis Rawlins' calculations" (1981e). Rawlins himself, by general consent, had nothing to do with sample selection. And according to Gauquelin, Kurtz presented him (G.) with the results of the first and second selections, as a *fait accompli*, during a visit to Buffalo on March 21, 1978. (Of Zelen's role, I have no information.) It seems fairly clear, therefore, that Kurtz and his assistants (F. Dolce and G. Harnden) had sole control over the first, second and third selections, statements to the contrary notwithstanding. The evidence is also, on balance, against there ever having been a prior agreement as to sources of data.

Rawlins had been opposed to CSICOP involvement in sampling from the beginning -- that is, without rules in advance in writing, and impartial judges. In correspondence, Abell (1981b) concurred that for Rawlins, such ideas were not "hindsight." Why, then, was Rawlins' advice ignored yet again? When Rawlins tried to raise these points in his 1979-80 contribution to the Skeptical Inquirer, his paper -- already the briefest -- suffered no fewer than twelve deletions. According to Rawlins, his views were "bowdlerized" -- including "my attempted statement that there had been deletions from the paper before publication and that these deletions were available from me at my address" (1981b). Perhaps I should remind the reader that it was Rawlins who calculated the 408 celestial sectors, designed and computed the expectation curve, and calculated the statistics for the entire U.S. tests; or, as Abell (1981a) puts it, "carried the lion's share..." (One would certainly not get this impression from the SI format, or its official press-release (in March, 1980) stating "The results of the U.S. test, by three scientists/scholars from three American universities, were announced by the Committee..." with "one by Dennis Rawlins elaborating on the ...results.") Be that as it may, Rawlins was dropped from the Fellows of CSICOP in October, 1980. (No official reasons were ever given, and according to at

least three Councillors, no balloting took place (Rawlins, 1981c)).

Perhaps I should also point out that Rawlins is and always has been a non-believer in the existence of a Mars effect; witness his scathing article in The Zetetic (1977). Indeed, it would be odd if a co-founder of CSICOP were otherwise! One of his criticisms of Gauquelin -- that he resorts to a "creme-de-la-creme alibi" (1979-80, p.29) -- is actually highly unfair. Apart from evidently not being consulted on the sources of data, Gauquelin's stipulation of "only the top professionals" is a very long-standing one (e.g., Gauquelin, 1960).

For whatever reasons, Rawlins was permitted to observe in the SI (1979-80, p. 29) that both KZ&A and Gauquelin resort to "post-hoc sample-splitting ploys." This is certainly true; the test's "design" seems guaranteed to produce such a result. Therefore it is obvious that no firm conclusion(s), respecting its outcome, are possible. However, there is no reason we cannot try to salvage something from the wreck by examining the relative validity of the post hoc samples and points. In this way, it may be possible to reach some tentative conclusions. Of course, much of this has already been covered above, and in the synopses of exchanges between KZ&A and Gauquelin. Some additional considerations are as follows.

Gauquelin (1979-80, p.33) has objected to the use of basketball players, since he had previously noted that they showed a very low Mars effect in his European sample. This is inadmissible, since he offers no independent evidence that basketball players should require less aggression, etc. than any other athletes. (In private he conjectures that physical factors of height and reach may be unusually important, but concedes that "top" basketball players should show a Mars effect.)

KZ&A (or rather Kurtz, since it seems that authorship was his as well) demonstrate (1979-80, pp.24 & 54) that Mars in the total U.S. sample appears in many unpredicted sectors more often than it does in predicted sectors 1 or 4. More interestingly, in Gauquelin's chosen 88, "Mars appears in sectors 7 and 8 more often than in sector 4, and as often in sector 10 as in sector 4" (p.54). In the first sample of 128, Mars appears in sector 10 more often than in 1, and 5,7,8 and 10 than in 4. However, Gauquelin could have replied that his published patterns of "planetary effects" (again, from as early as 1960) have always shown emphasis in sectors 7 and 10, as well as (though less than) sectors 1 and 4. Though Gauquelin could be faulted for failing to specify all four sectors, such a reply would not be technically ad hoc, since it was clearly not just designed at the time to cover this case. The same is true of his response to Kurtz's query, "why do not ordinary or moderately successful sportsmen show the Mars effect?" Gauquelin responds that "the relationship between Mars and success in sports is weaker than the correlation between Mars and the temperament. Highly successful champions very often possess what we describe as the 'Mars temperament.' Such temperament is not absent in less renowned athletes, but it is less marked and not more frequent than in non-athletic people" (1979-80, p.39). This sounds like a simply common-sensical conjecture -- the intervening variable between a valid Mars effect and professional success is obviously personality or "temperament." But since the early 1970's, the bulk of the Gauquelins' research has concerned planetary temperaments, using their own and others' personality measures. These results claim consistently higher correlations than those for planets and professions. (Of course, I need hardly add that independent replication, of a competent sort, is urgently needed (See Gauquelin, 1973; 1978b).)

There are more fundamental questions to be asked about Kurtz's selections. Nowhere that I could find does he directly answer Gauquelin's claim that athletes in the second and third canvasses, while still prestigious, are not as eminent as those of the first group. Again, Kurtz says that the team in Buffalo did not know the results of the data until the calculations were later performed independently of its efforts (1981e). But the spirit of this statement is contradicted by the fact that, according to Rawlins, he received the data to be calculated, and sent back the results (to Kurtz alone) in at least three separate batches (from autumn 1977 through autumn 1978) (1981c). In this context, the dramatic drop in the Mars effect over the three sub-samples --from 19.5% to 12.5% to a significantly low 7%-- may pose much less of a problem for Gauquelin (which *prima facie* it appears to do) than it adds to a host of reservations about Kurtz's sampling. (Similarly, almost amusingly, of the 83 names in the last "canvass," 54 are those of basketball players; are the implications more interesting for Gauquelin's hypothesis, or Kurtz's choice of sample?)

In fact, even Kurtz's chosen "181" cannot be cited as evidence against Gauquelin, since although its Mars effect is only 16.6%, its confidence limits include (besides the level of chance) his European finding of 22%. (See SI, 1979-80, p. 53, "Table 9"; the same point applies also to Tables 4,5,6 and 7.)

Finally, regarding the published material, examination shows that the majority of all sub-samples chosen -- particularly the undisputed ones -- while small and therefore of low power, do show a Mars effect in the direction predicted by Gauquelin's hypothesis.

In late 1979, while staying in California, the Gauquelins managed to obtain the birth data for 16 new Olympic gold medallists and 12 new "Notable Sports Personalities," all listed as such in the World Almanac (1978). This was enlarged by data for 11 athletes added to the 1978 edition of the Lincoln Library (KZ&A had used the 1974 ed.). Minus overlap, the result is a sample of 35 new "U.S. Star Sportsmen," with a Mars effect of 9, or 25%. (With the Olympic champions alone, it is 5 out of 16, or 31.3%) The Gauquelins comment, "Since we explicitly predicted in our former paper (SI, Winter 1979-80) that the Olympic Champions and the 'Notable Sports Personalities' should display the Mars effect, it is obvious that this new result is not due to an a posteriori selection of the data" (1980).

This information was sent to K. Frazier (editor of the SI) in early 1980, but refused publication. (Copies of the birth data replies were sent to KZ&A.) In fact, the last word on the U.S. test again belonged to CSICOP Fellow L. Jerome (SI, Fall 1980, pp.85-86). In a letter, he offers his "congratulations" to the Committee, accusing (en route) Gauquelin and Rawlins of incompetence and the latter (in a different, and irrelevant, context) of "censorship." This really needs no comment, but I will add that of Abell, in correspondence with Kurtz (1981d): "We do not endorse Jerome's statistical arguments. (Frankly, I don't think Jerome knows what he is talking about.)" --and point out that once again, we end with a glaring gulf between the public position of CSICOP and its members' private sentiments, let alone (as far as we can ascertain) the truth.

New European Replication/Synopsis

The documents for this are the same as cited above for the U.S. test. In SI (1979-80, pp.39-40), Gauquelin describes an offer to KZ&A and Rawlins (in a letter of Nov. 10, 1978) to assist in collecting and analyzing a new sample

of athletes in Europe, where birth data is easier to obtain than in the U.S., "under the entire control of the Committee." Despite receiving no answer, he repeated the offer in a trip to the U.S. in April, 1979, again to no avail. The Gauquelins therefore carried out the test themselves; the data and results are in 1979a. The 432 athletes collected come from 7 countries, and show a Mars effect of 106, or 24%; this is significant at .001. There were no significant national differences, and a control of non-famous athletes apparently showed no Mars effect.

KZ&A reply to Gauquelin (pp. 57-59) discusses Gauquelin's 1979a, a copy of which they received. Their first three objections, relatively minor, will be discussed below. The major point they raise is that in his selection for the French part of the new sample, Gauquelin used the names of gold, silver or bronze medallists at the Olympics, the world championships, or European championships; whereas in his post hoc sample of American athletes, he retained only gold medallists. Applying Gauquelin's "new" criteria to the U.S. sample results in 18 champions, with a Mars effect of only 3 (16.7%). KZ&A "wonder...whether the criteria for the selection of the 432 "greats" and the 423 "lesser" athletes were established before or after the Mars sectors were calculated" (p. 59). They go on to "submit that the original European study of 2,088 sports champions should be reexamined..." since the criteria seem to be now more restrictive.

In his response (SI, 1980, pp. 58-62), Gauquelin reminds the reader that he had offered control of the test to the Committee before it was performed. He also indicates that no selection at all was used for the athletes from Italy, Germany, Belgium, Holland, Scotland, Spain and Luxembourg. "...it is only for the French part of my sample that I had to establish a selection, because, by an understandable chauvinism, the author of the French Dictionnaire des Sports [Denoel, 1973; the source used] had listed names of French sportsmen for nearly half of his book..." (p.61). In Oct., 1979, copies of the documents were sent to Kurtz with a request for his (Kurtz's) selection of the French players of international status. Gauquelin points out that if they were all really "famous," the "density of French famous champions would be incredibly high compared with other European countries"(pp.61-62). In any case, adding the 423 "lesser" still leaves an overall Mars effect of 20.7% -- "a still significant figure, but the significance comes entirely from the famous athletes" (p.62).

In their final rejoinder (SI, 1980, pp.62-68), KZ&A say that Gauquelin's "432" contain 31 individual and team Olympic gold medallists, with a Mars effect of only 4, or 12.9%. They comment that this figure is "surely surprisingly low if Gauquelin's theory is correct, since by his criteria Olympic gold medallists are the crème-de-la-crème of sports champions" (p.64).

Adding to these "31" the names of 25 silver and bronze medallists in the new European sample produces a Mars effect of 16.1%. And of the 13 European Olympic champions listed in The Olympic Games (ed. Killanin and Rodda; Macmillan 1976), none displays a Mars effect. KZ&A say that they have not been able to completely analyze Gauquelin's new replication "because we have not yet received all of the data from him" (p.65). They are referring to the list of the 423 excluded French athletes. Nonetheless, they go on to register several objections to various members of Gauquelin's "original" 2,088 -- i.e., that they are not, for example, internationally famous. They conclude, in part, that "either the original European study is invalid, because it uses looser criteria for the selection of champions, or the new study is invalid, because it is too restrictive in those it includes" (p.66).

New European Replication/Assessment and Follow-up

As with the U.S. test above, this evaluation will draw partly on unpublished material, and will not attempt to cover points that have (in fact, not apparently) been satisfactorily answered in the exchanges just described.

First, to cover some minor published points, KZ&A had noted that Gauquelin had experienced difficulties in obtaining data from German and Spanish birth registries (therefore perhaps introducing undesirable selectivity); they neglected to mention that he obtained almost all the data requested from Italy, Belgium, the Netherlands and Scotland (SI, 1980, p.61). They had noted that 21 individuals born after 1950 were not excluded, but failed to mention that Gauquelin had explicitly pointed out that the Mars effect disappears with these births. And their objection that his requirement of international success is too narrow, since it would (for example) "exclude almost all baseball and football players" (SI, 1979-80, p.58) is absurd since it is trivially true; baseball and football being almost uniquely American sports, there are no international competitions in these sports. (Gauquelin nowhere rejects these sports, as such, as unsuitable for showing a Mars effect.) Finally, most of the objections to various individuals in the "2,088" (SI, 1980, p.66) -- it should be stated for the record -- apply to members of the Comité Para's sample, rather than those collected by Gauquelin.

Correspondence relative to this matter, over 1980-81, runs basically as follows. Kurtz has repeatedly requested from Gauquelin a list of the excluded or "lesser" 423 athletes, with a list of their Mars positions. At the same time, he has asked for a list of the Olympic Champions and World Almanac "Notable Sports personalities" which resulted in Gauquelin's new sample of 35 (see above), including refusals. (This covers letters dated Feb. 6, 1980; Apr. 22, 1980, May 28, 1980, and Oct. 16, 1980.)

In an important letter to Kurtz dated July 27, 1980, Gauquelin replies as follows: (1) he would be glad to send the requested information on the new U.S. athletes, after Kurtz has established himself the list of Olympic champions and W. Almanac "Notable Sports Personalities." "Doing this, the choice of the names of the athletes cannot be questioned afterwards." (2) In response to Kurtz's request for information on the 423 "lesser" athletes, Gauquelin outlines the following proposal, which is worth quoting in full.

"1 -- You establish the list of all the athletes mentioned in [the Dictionnaire des Sports, 1973 -- a copy of which Gauquelin had airmailed to Kurtz in the fall of 1979, at the latter's request] who are American, Belgian, Dutch, French, West German, Luxemburgian, Spanish and Scottish (the time of birth is not recorded in England and in Ireland). Doing so, you will have athletes listed belonging to my first sample (...1955), to my second sample (...1960), to the Belgian Comité Para sample (1968), to your U.S. sample (1979), to my third sample (Scientific Documents No. 7, 1979) plus a group of less renowned French athletes I did not include in my third sample of outstanding sports champions.

"2 -- After our mutual agreement on this list of names, indication will be given concerning the place, date and time of birth of the athletes listed. Justifications will be given when this information is not available for some names of the list.

"3 -- After our mutual agreement on point no. 2, the position of Mars at the birth of these athletes will be calculated by you. The

actual distribution of Mars will then be compared to a theoretical distribution estimated by G.A. Abell and his collaborator Albert Lee (see G. Abell's letter of May 3, 1980).

"4 -- After our mutual agreement on point no. 3, a statistical analysis will be performed --using for instance the chi-squared test -- comparing the actual number of athletes born with Mars in 'key sectors' with the number of athletes born [with Mars] in the other sectors. Several analyses could be done: a) on the entire sample; b) on the entire sample without the athletes of my first group...; c) on the entire group of athletes without all the French ones, since you question the French selection of data; etc.

"I think procedure fits very well with all your concerns about the selection of the sample and all possible contradictions in my research. I hope you will accept my suggestion and I will appreciate receiving your list of names of the athletes from the Dictionnaire...at your earliest convenience."

Copies of this letter were sent to Abell, Zelen, Frazier and Rawlins. It also notes non-publication of the "Note" concerning the 35 new U.S. champions and Abell's confirmation of Gauquelin's theoretical calculations of sectors.

A brief reply from Kurtz (Oct. 16, 1980) makes no mention of either suggestion 1) or 2) above. This was the last communication received by Gauquelin from Kurtz until a letter dated March 18, 1981. During this period, Gauquelin unsuccessfully demanded a brief reply to KZ&A's final SI piece (to Frazier, Jan. 7, 1981); and repeated his proposal to Kurtz in letters of October 30, 1980, and Jan. 7, 1981 (registered).

Gauquelin's Oct. 30 letter also points out that KZ&A's "Rejoinder" (SI 1980) asserts that European Olympic gold medallists (see above) do not display the Mars effect. However, Kurtz had evidently forgotten 24 cases -- of whom 11 show a Mars effect, i.e., 46%. (A further letter of Feb. 3, 1981 suggests that Kurtz establish himself "a complete list of all the U.S. and European Olympic champions (gold medallists)since the beginning of the Olympic Games.")

This last letter also reiterates that the proposed test involves "no selection at all... I asked you to establish the complete list of the U.S. and European athletes of this volume whatever they are, "great" or "lesser great": is that clear enough?"

Kurtz' eventual response was a registered letter of March 18, 1981. In it he (1) repeats his request for information on the 423 "lesser" French athletes; (2) stresses his opposition to any selection from Who's Who's or Dictionary's; (3) again asks if the same rigorous criteria for selecting sports champions were used in the study of 2,088 champions as in the new European study or the American study, according to Gauquelin's requirements; (4) says that the published point against European Olympic gold medallists primarily concerned the second study and not the first.

At this point, I must comment that the reader of the SI is left with the impression -- intentionally or not -- that European gold medallists en tout do not bear out Gauquelin's hypothesis, or the strong Mars effect noted in American gold medallists. (Gauquelin "does not mention, much less explain, this difference" SI 1980, p.64.) This is misleading, as is shown by adding KZ&A's "31" (from the new European sample) to Gauquelin's "24" -- the total Mars effect is 15, or 27.2%. Secondly, Kurtz's response to Gauquelin's

proposal is wholly remarkable. In view of the latter's repeated and detailed point that no selection whatsoever is involved, how is it possible for Kurtz to reply (I am not at liberty to quote) that if an athlete appears in a Dictionary, that is sufficient criterion for selection as a great champion, and that to cull still others introduces subjectivism? (In addition this again fails to meet Gauquelin's hypothesis in terms of degree of greatness, although Gauquelin himself is willing to forgo any selection from the Dictionnaire; and if selection necessarily introduced serious (non-trivial) subjectivism, science would be in a lot of trouble! Subjectivism is, of course, avoidable through the mechanical application of objectively assessable conventions.)

Gauquelin (Apr. 2, 1981) again repeated and re-described his proposal at length, and offered to meet with Kurtz on an upcoming trip to the U.S. Kurtz (May 28, 1981) responded by repeating his two requests and opposition to selection; he refers vaguely to the possibility of a new test with fresh data but makes no mention of Gauquelin's proposal. A long letter from Gauquelin (June 1, 1981; registered) reviews the situation, raising "several points where the truth was hidden or seriously distorted." There is no need to go into details here, or those of Kurtz's reply (June 26, 1981; registered), since they have been amply covered above. (Incredibly, though, Kurtz states, regarding the Dictionnaire des Sports, that he sees no basis for making a selection from that.) And in a further Letter, sent to many persons on June 24, 1981, Kurtz states that Gauquelin insists that he (Kurtz), Abell and Zelen go through the Dictionnaire and prepare a list of those they think are truly "outstanding." The reader will appreciate that I am now at a loss to rationally explain such statements. But whatever the explanation, there can be no doubt that they represent a very serious distortion of the truth of the matter.

Kurtz continues that this is a Catch-22 situation; KZ&A want to see the basis of Gauquelin's decision to exclude 423 names. Gauquelin refuses to send his list of 423 so-called "lesser" champions until KZ&A make up a list. So, we are apparently in a very similiar situation to that at the end of the U.S. test -- due to the "unfortunate" way it was conducted, we now cannot conclude anything very much about the Mars effect. And once again, I must respond by saying, (1) was this necessary? and (2) what can we conclude, based on careful analysis of the results, such as they are?

As before, the answer to (1) is, no. Responsibility for the post hoc morass following Gauquelin's new European replication rests firmly on the shoulders of Kurtz (and, to whatever extent they were involved, Abell and Zelen). Gauquelin's letter of Nov. 10, 1978, and sent to all parties concerned, states clearly: "We would be happy if you would accept to entirely control this experiment, that is: - check the choice of our selected list; - verify the answers received from the birth registers; - perform yourself the astronomical calculations for Mars corresponding to the births... We would be in a position to send you a list of famous European athletes with hour of birth in one month. We are looking forward to your response on this proposal." Yet after they have received the data and are in possession of the Mars sectors, KZ&A not only quarrel with Gauquelin's selection; they bring into question Gauquelin's original sample of 2,088 -- despite having verified this sample's integrity in a document ("Examination of the Sports Champions Data"), signed by Kurtz and M. & F. Gauquelin, in Paris on June 24, 1977. This document states, "Paul Kurtz examined thoroughly each document, and declared himself satisfied by the objectivity of our procedure." And above all, KZ&A have persistently ignored and/or misrepresented a clear proposal to decide the matter.

Unfortunately, a recent letter to Professor Every Schatzman from Kurtz (June 24, 1981) continues this process. He repeats that he believes the only objective basis for the selection of sports champions to be the Who's Who's and Dictionnaires of renowned sports champions, and that any other selection is arbitrary. In conclusion, on this point, I can only endorse the words of Gauquelin's most recent letter to Kurtz at the time of writing (July 17, 1981; registered): "...it is clear that my proposal of July 1980 fits perfectly well with your ideas on how to verify the Mars effect. That is: to take all the names listed in the Dictionnaire des Sports without making any selection...at all. -- Now that you state that the use of the Dictionnaire in its whole is the only way to remain objective, you cannot refuse my offer."

What of the implications of the new European replication -- (2), above -- for the existence or nonexistence of the Mars effect? Obviously, it awaits competent independent replication. That said, KZ&A's objections seem, in the main, weak. To put it another way, Gauquelin's rationale for his French selection, and the decision to so select, is plausible: his answers to most of KZ&A's criticisms (most of which answers remain unpublished) are satisfactory; and one notes that even addition of the 423 "lesser" athletes to the whole sample results in a still significant figure (20.7%). Even KZ&A's critical comparison of the "2,088" with the new "432" is questionable (aside from being glaringly post hoc); its essence is that Gauquelin's former sample and results contradict the later in using looser criteria, but to invalidate the result of significance for both sets-- or, rather, to use that result to invalidate Gauquelin's sampling-- would surely call for more radical discrepancies than are apparent. True, the later sample has a higher Mars effect (24%) than the earlier (22%), but could not Gauquelin adduce the occasional weakness of the Comite' Para's criteria? In any case, the point is: there is and has been, for over a year, a way to settle this; but Kurtz, with the tacit support of the rest of the Committee, has chosen instead to engage in further fruitless post hoc haggling.

Conclusions/CSICOP

I don't think I need to stress how badly the Committee has handled the investigation of the Mars effect; the facts above speak for themselves. Their work could now best function as a model and a warning of how not to conduct such investigations. Given the ample internal (Rawlins) and external (Gauquelin) warnings that went suppressed or ignored, it is even difficult to accept protestations of "good faith" and "naivete" (Abell, 1981c). Rawlins and Gauquelin are in fact the only two major figures to emerge with scientific credibility intact. It seems to me that this situation must call into question any further (unrefereed, at least) CSICOP involvement in research on the Mars effect, and possible other "paranormal" areas.

I earlier mentioned that there are occasions in the history of science when a "sociological" explanation seems called for. This seems to be one. It would have to take into account such considerations as: the nature of the claims being investigated; undue involvement of scientists with media and publicity, or perhaps conversely, unique (especially in America?) pressures of public-relations on science; considerations of where power resides in such an organization, and how it is exercised (financially? publishing rights?); and lastly, how information circulates, or fails to circulate. (Of SI policy, we are now aware; readers of SI alone are not so lucky. Also, there are a number of "big name" figureheads on the masthead; are they aware of CSICOP behavior, which they presumably support?)

Of course, it could be argued -- and has been (e.g. by Abell, 1981c) -- that the entire testing of Gauquelin's work was a purely "personal experiment," and nothing to do with the CSICOP. This would involve believing that these experiments "just happened" to be run by the Chairman and Fellows of the CSICOP, and be published in its official organ. It would also overlook the fact that Rawlins was paid (starting Oct. 20, 1977) with CSICOP checks for his calculations; and contradict Abell's earlier (1978b) description of "the subcommittee that agreed to look into the Mars effect on behalf of the Committee." Finally, such backpedalling is unflattering to CSICOP; if true, it implies that an organization whose much-publicized *raison d'être* is "...Scientific Investigation..." has been in existence for five years without conducting one major investigation. The scientific quality of its work, if we refuse disownment, is something that thankfully needs no further comment.

Conclusions/The Mars Effect

On the strength of the work we have covered above, the Mars effect on balance stands as corroborated. That said, there is an urgent need for truly independent and competent replication -- with procedures in detail agreed-upon in advance and in writing, and conducted double-blind, and/or without any possibility of interference on one side or the other before results were computed. Such a test could use either, or both: (1) a re-analysis of the data such as Gauquelin's 1980 proposal involves; (2) collection and analysis of fresh data, such as a committee of French scientists is apparently considering.

I believe also that it is high time that Gauquelin's rather more interesting and fruitful research in the field of "planetary temperaments," i.e. personality, received consideration. (See Gauquelin, 1973, '74, '77, '78a,b.) The only independent (or partly so) study here was that of the Gauquelins with S.B.G. Eysenck (1979), which found strongly significant planetary effects for extraversion and introversion in line with the hypothesis' prediction. This is a tantalizing result, which begs for replication and further investigation. (Regarding the difficulties of doing so, see Goodstein and Brazis (1970), who found that bias among psychologists regarding "astrological" findings exists at a significant level.)

Lastly, it may even be time to start thinking, at least, about how a genuine planetary effect(s) might come about, and what its existence might imply. The implications would be considerable -- for history and philosophy of science, for epistemology, for biology and physics, and perhaps especially for the interaction of the last two. (Gauquelin's findings involve the 24-hour or "circadian" rhythm: easily the most powerful of human biological cycles, and one not very well understood.) Certainly, there are very good reasons for scientific conservatism; one does not sacrifice a hard-won body of knowledge for a will-o-the-wisp. But science also certainly does not progress by ignoring or suppressing opportunities to extend that knowledge. A priorism, or "unthinkability," is no excuse, as history demonstrates.

Personally speaking, I do not find it completely unthinkable that a (roughly) four-thousand-year old human intuition and some of its forms should contain some empirical truth. As is well-documented, that was Kepler's position; and a contemporaneous group of English scientists, three-hundred years ago, attempted a scientific reform of astrology (Bowden, 1974). But statistics and psychometrics were still in infancy, and the attempt died. With the possibility, the time has now come to give "neo-astrology" a fair trial.

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(R = Registered.)

ADDENDUM

Since writing this article, a welcome addition to the literature has been brought to my attention. It is a review-discussion of Gauquelin's Cosmic Clocks (1973) and Cosmic Influences... (1976), by H. Krips, in Erkenntnis 14 (1979), pp. 373-392. Krips discusses the Zelen test at some length. He asks, "What is the response of Gauquelin's critics to this positive result? Do we find them admitting that their "bold conjectures" have been "falsified"? No -- there are several strategies they adopt to save their hypothesis" (p. 387). In an analysis which agrees closely with my own, Krips concludes of "Zelen et al." that "none of their (dare one say "ad hoc") tactics to avoid the positive results of their own test are successful" (p. 389).

Since Krip's article is a model of clarity and thoroughness rare in the scientific literature on astrology, it is also interesting to note his final comment -- "In particular there seems little grounds for the anxiety of Bok et al., that the study of astrology -- at qua Gauquelin's theory -- is a sign of the dawning of a new age of "irrationalism and obscurantism" (p. 391).



SPECIAL POSTSCRIPT & UPDATE

Since receipt of Mr. Curry's manuscript and the commentaries that follow, a number of events have taken place. CSICOP's journal *The Skeptical Inquirer* (Winter 1981) has published a Council statement in response to Dennis Rawlins's charges, another statement by Profs. Abell and Kurtz, a letter from M. Gauquelin with a reply from Abell and Kurtz, and a 6-page article (of further attack) by Dennis Rawlins with two pages of introduction by editor Kendrick Frazier. As advertised in that issue, CSICOP will send out a packet of reply materials to interested parties who send three dollars to CSICOP (Executive Council; CSICOP; 1203 Kensington Ave.; Buffalo, NY 14215). This packet includes the article "Crybaby" by Philip J. Klass, and "The Status of the Mars Effect" by George Abell, Paul Kurtz and Marvin Zelen. ZS readers should find careful comparison of these statements with the other published documents, especially "sTarbaby," of great interest. Readers should also find the article by Jeremy Cherfas (who is associated with the British "branch" of CSICOP) in *New Scientist* (Oct. 29, 1981) and the letters following from Gauquelin (Jan. 7, 1982), Kurtz (Feb. 11, 1981) and Curry (March 4, 1982). It is our understanding that several articles dealing with this controversy are now being prepared for publication in the international media; so we have not yet seen the end of this affair.

The next issue of *ZETETIC SCHOLAR*, in addition to whatever reactions come to us on this issue, will include (1) a special outline/synopsis of the controversy along with an evaluation by me, and (2) a special report on a survey of CSICOP "members" made by Prof. R.A. McConnell immediately following the publication of Rawlins' "sTarbaby" article (this survey is itself a matter of some controversy). We also hope to publish comments by CSICOP members, especially from members of the CSICOP Council, and I particularly urge those who support CSICOP in this controversy to participate in the dialogues scheduled.

-- M. TRUZZI



CRITICAL COMMENTARIES



COMMENTS BY MICHEL GAUQUELIN:

Patrick Curry's careful analysis is a successful effort to bring some clarity to the Mars effect controversy. I am especially relieved to see my repeated letters to Paul Kurtz quoted by Curry because it was tedious work to do so and, up to now, not at all rewarding. Though very different from the recent Dennis Rawlins' article ("Starbaby," *Fate*, October 1981), Curry's appraisal leads to the same devastating conclusion concerning the way the CSICOP actually ran the "scientific investigation" of the Mars effect. I think the exposure of the CSICOP's policy, through all the documents fully and accurately covered by Curry, is extremely revealing.

The dispute, under its apparent confusion, teaches us some positive points regarding the growing evidence that the Mars effect is an actual fact. I would like to successively consider these positive points in favor of the Mars effect.

1. Our expectation curve of Mars is accurate.

As soon as 1957, in our book *Méthodes*, we calculated the expectation curve of Mars and demonstrated that 17.1 percent for Mars being in key sectors 1 + 4 (rise and culmination sectors) is the right figure to expect.^{1*} This figure was questioned by Jerome and the Belgium Para Comité. I think it now established "beyond any reasonable doubt" that we are right on this point which was - it should be remembered - the very origin of the CSICOP's involvement in the Mars effect. In fact, Rawlins' memorandum (1977), the outcomes of the Zelen test (1977), and Abell & Lee's empirical checking (1980) all demonstrated that 17.1 percent is indeed the right value. Even Paul Kurtz, in his last letter to date (to all Fellows and Consultants of the CSICOP on September 21, 1981) wrote: "we perhaps should have stated (after the outcomes of the Zelen test). that the theoretical expectation was close to chance (or more precisely 17.1 percent for the key sectors). This was a point in dispute with the Belgium Para Comité. We did not deny this point, and indeed assumed it in the subsequent American test." Thank you, Professor Kurtz. I think your statement - though a little late - definitively settled this important point.²

2. The Mars effect was found and replicated using clean samples.

Since there is no more dispute about the expectation curve, all the core of the debate now lies in the correctness of the samples of athletes. If the samples are clean and show the Mars effect, the Mars effect is

* Please see the notes at the end of these comments.

demonstrated. Dennis Rawlins, and later Kurtz-Zelen-Abell, questioned my original sample. Well, I think it is a legitimate interrogation to wonder about how the data were collected. I am not offended at all by these reservations as long as they are followed by a careful and honest scrutiny of the data. I am myself so ready to find this point crucial that, as early as 1955, in my first book L'Influence des Astres, I published all the birthdata I gathered and provided all my sources of information. Fifteen years later, in 1970, my laboratory published the birth and planetary data on 2,088 sports champions and a detailed account of all the bases under which they were collected (Series A, Volume 1). But I cannot accept that - after a careful examination - the objectivity of our sample still remains questioned.

Interestingly, it is Paul Kurtz himself who thoroughly varified our sample in 1977, several months before the publication in The Humanist of the Zelen test results. He (and Frank Dolce) compared the original entry listed in the two directories used for collecting the sample with the birthdata published in our volume on sports champions. He visited my laboratory on June 24, 1977 (where he spent an entire day, to his credit!). I was able to answer all his questions to his satisfaction. I provided him the two hundred original documents I received from the registry offices he wanted to examine at hand. A text of three pages, called "Account of the meeting of Paul Kurtz with Michel & Francoise Gauquelin in Paris, June 24, 1977: examination of the sports champions birthdata" was signed by him and us and sent to Marvin Zelen and George Abell. In fact, it is also Kurtz himself, in The Humanist (Nov/Dec 1977) who stated that he "inspected the Gauquelin's archives and was impressed by the meticulous care with which the data had been collected." Incredibly, Kurtz's last letter on September 21, 1981, to all the Fellows and Consultants of the CSICOP, completely contradicts his own 1977 statement! He now claims that before the Zelen test, he (and Zelen & Abell) "did not question Gauquelin's integrity or raise the question whether his original data of the 2,088 sports champions were correct...we gave Gauquelin the benefit of the doubt..since the American test our question to Gauquelin (which still has not been answered) is: on what basis did he select the original sample of 2,088 sports champions?" Kurtz's last statement is twice untrue. He did not give me the "benefit of the doubt" at all before the publication of the Zelen test results. It was just the contrary; and I was able to answer all his questions about the basis of our first sample.

Let us now examine the validity of our second sample of champions. Before running the test, I actually asked Kurtz, by my letter of November 10, 1978, to fully collaborate and to control it, also with Zelen and Abell. I got no answer. So I ran the test myself. All the birthdata and bases of this sample were published by my laboratory in 1979 (in our Series D, Volume 6). I sent a copy of this report ot Kurtz (and others), and I did provide all the opportunities for him to verify the accuracy of this sample. For instance, I sent to Kurtz, by air mail, the huge Dictionnaire des Sports (published in 1973) basis of the experiment. Nevertheless, in The Skeptical Inquirer (Winter and Summer 1980), Kurtz-Zelen-Abell accused me - without any proof - of removing "423 famous champions" from this dictionary. My reply to Kurtz was a proposal (my letter of July 27, 1980, quoted by Patrick Curry in his article): Let Kurtz himself consider all the names of the athletes listed in this dictionnary without

any selection at all and see if the Mars effect still shows up when all the athletes in the book are included (the Mars effect does, by the way). In his article, Curry clearly shows how Kurtz stubbornly declined to answer my repeated letters urging him to carry out the experiment and how Kurtz only shammed understanding nothing despite the simplicity of my proposal.

Now, please keep in mind the Belgian Para Comite sample. Members of this Committee agreed in 1967, before all calculations, upon a list of names of sports champions; and they successfully replicated the Mars effect. The fact that they waited seven years before publishing a report in which they covered-up the meaning of their own results - falsely questioning our expectation curve of Mars - is pretty good evidence, I think, that the Belgian Para Comite was not in a disposition of mind to use an improper sample of athletes likely to show such a (for it) repellent Mars effect.³ I think it is fully demonstrated that our samples, and the Belgian Para Comite sample, are clean. In all of them, the Mars effect very significantly shows up.

3. Despite its many defects, the US test came out positive for the Mars effect.

According to Kurtz-Zelen-Abell, the US test is based on a genuine sample and it came out negative for the Mars effect. For my part, according to my own experience, and after the reading of Rawlins' revealing "sTarbaby," I think we can raise some serious questions about the way the US test was run. First, Kurtz ran the test alone without asking my agreement upon the choice of the volumes used. I was not informed, even verbally, of the experiment before it was entirely done. Why did Kurtz behave like this? Rawlins shows he wanted to handle and to control the data and the results in his own way. Why did Kurtz send Rawlins the first set of data secretly, saying that he wished a private advance look at how the computation was going to come out? A sentence, in Rawlins' article, is revealing: "at one point (after 120 names) I told Kurtz by phone that the keysector score was now at 22 percent. He groaned." Understandable: it was exactly the Mars effect hit-rate which has been predicted. Why, after that, did the extra data of athletes added to the US test by Kurtz come out so drastically against the Mars effect that a statistical analysis shows that mere chance cannot be invoked for explaining the result? Curry also points out in his article a "dramatic drop in the Mars effect over the three sub-samples." In "sTarbaby" Rawlins tells us: "No sooner was this task finished and the American test supposedly completed than Kurtz phoned me up and said oops, we accidentally missed a lot of names..I returned to San Diego some weeks later. The last 82 names came in at summer's end." It is interesting to know that these 82 additional athletes - the last of the three sub-samples - show a hit-rate of Mars in key-sectors of 7 percent only, instead of the 22 percent found in the first 120 names: a very significant "anti-Mars effect" indeed (in this additional sample, Mars is in key-sector 6 times; chance predicts 14 times. That gives an anti-Mars effect significant at the .02 level). We have to find out an explanation for the striking statistical difference between the 22 percent score of the first data and the 7 percent score of the remaining "accidentally missed" 82 names. In my letter of November 10, 1978, to Kurtz, I looked for an explanation on this point. He did not answer my letter.

Kurtz also claims that he did not get any answer at all (even refusal) when he requested the American data on athletes from eight states; among them, Texas. Kurtz had not heard from this state and he was deprived of 65 cases of athletes belonging to his first sub-sample and 96 belonging to his other sub-samples. A big loss. For my part, when I requested data from Texas, I received a fairly good percentage of positive answers. Let me be clear: I do not claim that Kurtz concealed Texas data (or others). I just think it is unlikely he did not receive any answer at all from this state. Anyway, this is the kind of thing we should be careful about before accepting Kurtz's data without concern.⁴

More important: genuine or not, the outcomes of the American test tend to vindicate the Mars effect. In this test, the more famous are the athletes, the higher is the Mars effect. It is a fact that Kurtz's first selection sample shows a significantly stronger Mars effect than the other sub-samples. It is also a fact that the first sample contains much more outstanding athletes than the second and the third ones. I have written evidence of that. It is a document Kurtz gave me on March 21, 1978, when I visited him in Buffalo.⁵ The reading of this document clearly shows that my analysis published in The Skeptical Inquirer (Winter 1979/80) where I compare the results of the first sub-sample with the result of the others is not a post hoc interpretation on my part but just follows what Kurtz did: he first took the well-known athletes and afterward took the less renowned ones.

We can assume, therefore, that the American test is also in favor of the Mars effect (and it would probably have been much more clearcut, like the Belgian Para Comite test, if it had been carried out under better scientific conditions).

4. The Mars effect should be tested like any other possible "normal" phenomena.

Please try, for a moment, to imagine that the Mars effect might be true. That does not mean the triumph of the Occult against Science in a battle-field! I am worried about the tendency shared by too many members of the "scientific investigation Committees" that one experiment is always decisive. It is not scientific to think that we may proudly "win" or ignominiously "lose" according to the outcomes of only one experiment. I experienced that with the Belgian Para Comite. Its members did not tolerate "losing." They took their positive replication of the Mars effect as an offense and not as an intriguing fact which needs further investigation. The same thing happened when Kurtz-Zelen-Abell "lost" the Zelen test. And, recently, Philippe Cousin, editor of the French magazine Science et Vie and member of the French Para Comite, requested that I write a protocol for a new control of the Mars effect using a challenging tone!

I am also worried about the rigid ideas people have about the Mars effect. I never claimed, for instance, that the Mars effect on sports champions should always be at 22 percent. It is obvious, for any trained statistician, that this value may vary according to the selection of the sample, the speciality of the sport, the size of the group and...pure chance. This percentage could be higher or lower than 22 percent. It does not really matter. The only specified hypothesis of the Mars effect is: famous athletes tend to be born significantly more often with Mars

in key sectors (frequencies of the rise and the culmination sectors added) than the non-champions.⁶ In a similar manner, when I assume that it is better to investigate the Mars effect on births that occurred before 1950 and not after, I don't mean that the effect should be always found before this year and never after it!

All scientists agree that a "normal" phenomenon should appear, and be replicated, under certain precise conditions. For the Mars effect, we know some of the conditions, but we are far from knowing all of them! It may be possible that, one time, in some country, the Mars effect may not be observed. That does not prove that the statistical evidence found in several other countries are automatically destroyed. And, if we accept, for a moment, that the Mars effect is not an awful occult phenomena, we can feel justified in looking for the best results according to our past experience on the subject. That is exactly how all scientists in all disciplines work, and that is the only way to make progress in their fields (if basketball players, at first, do not seem to display the Mars effect, it is justified to leave them when we are attempting a replication; despite my warning, Kurtz gathered a large sample of American basketball players for the US test who display, as did the French ones, the lowest Mars effect among various sports specialities. Is that a failure to replicate the Mars effect or a success for one of my predictions?)

Psychological and sociological implications from one country to another - or inside the same country at different periods of time - could strongly modify very well accepted "normal" phenomena. No scientist denies that. But, let us imagine that we assume, for instance, that the Mars effect among American athletes could be weaker than the effect among French athletes of the same level of achievement (because sports is far more important in the USA than in France and, consequently, gifted American people have many more opportunities to succeed in sports than French ones who are obliged to fight for themselves; in France, sports is considered insignificant in the high schools and in the universities). But, if we would hypothesize that, skeptics immediately think that we are looking for a loophole in case of a failure to replicate. They will react like this because the Mars effect looks like an impossible anomaly.

But let us take another example: the daily rhythm of birth. Nobody denies that there is a natural nychthemeral curve of birth despite the fact that this curve may present a completely different pattern according to places and times, especially since the development of induced birth techniques. The seasonal rhythm of birth itself, which is considered as a "normal" fact by every scientist, shows surprising discrepancies from one country to another. Recently, two English scholars used the English seasonal curve of birth as an expected one for studying an American group of professionals. But, amazingly, they were mistaken because, for the same years and the same geographical latitude, the English curve, with a maximum of births in spring, is quite the opposite of the American one which shows a maximum in early fall! Which "rationalist" would be ready to consider this lack of replication between England and USA as definitive evidence that there is absolutely no seasonal effect in human births? None. Because they are looking for a "rational" explanation of this lack of replication (an explanation not

found yet in this case, by the way).

If we consider how little we know about the Mars effect at birth compared to the daily and seasonal effects on births, we all must show some modesty before interpreting a Mars effect result whether it seems a success or a failure to replicate. We should also remember that the Mars effect among sports champions represents only a tiny part (less than 5 percent) of all the statistical evidence for the planetary effects at birth we have published over a quarter of a century!

5. It is "high time" to test our work on planets and personality.

This will be my last point. I heartily endorse Curry's works that it is "high time" to conduct some replication attempts on our findings on personality and planets. Consider, too, that after the publication of my book Les Hommes et les Astres in 1960 we left the study between profession and planets and devised a very objective "character-traits methodology" which not only gives considerable stronger results⁸ but offers more opportunities for others to replicate our findings.

In this kind of research, neither the profession nor the standing matter; only the character traits of the subjects. So any professionals, famous or not, could be analysed in the same way (and all professions together) provided only that their birth occurred naturally and that their character traits are sufficiently well defined in a homogeneous series of biographies.

Over more than ten years, we have tried to attract the interest of the psychological community to our work on planets and personality. But, as Curry points out, there is a strong prejudice against "neastrological" claims among psychologists. There is a notable exception, however, the leading English psychologist Hans Eysenck and his wife Sybil. Comparisons between Eysenck personality dimensions and the planetary temperaments showed very promising results with European subjects⁹ and this was successfully replicated with American ones very recently.¹⁰ The last word about the planetary effects could more likely be found through these new research directions.

As for the Mars effect, I think the demonstration is already done and can not be easily killed.

NOTES

1. M. & F. Gauquelin (1957): Méthodes Pour Étudier la Répartition des Astres dans le Mouvement Diurne. This book has a foreword written by a trained statistician, Prof. Jean Porte, administrator, French National Institute of Statistics, Paris (and a disbeliever in the occult). In his foreword Prof. Porte states: "I looked for errors in Gauquelin's methodology and I was not able to find any". The book was generously offered to any interested members of the Belgian Para Comité and of the CSICOP. A careful and unbiased reading of this book should have made the Zelen test unnecessary.

2. As for our computations of the actual frequencies of Mars in sectors at the birth of athletes, they were checked by several people including Belgian Comite Para members, Rawlins, Abell & Lee, etc., who did not question their accuracy.
3. About the Belgian Para Comite cover-up, see the comments of Prof. de Marre in this same issue of Zetetic Scholar. It must be remembered that the Zelen test was based not only upon our original sample, but also on the Comite Para sample.
4. In our European samples, we are in position to justify much more than Kurtz does with his American test because it is easier to work in Europe, especially in France, than in the USA (and, consequently, it is far more difficult to conceal some cases if one would want to). In fact, according to my own American experience, positive answers from US registry offices are so chaotic that it is very difficult to provide absolute evidence of the perfect objectivity of any collected sample.
5. I sent, in due time, a photocopy of this Kurtz's document called "Selection of samples of American Sports Champions (I - First selection process; II - Second selection process)" to all interested parties.
6. Over twenty years, I never changed the specified hypothesis of the Mars effect. It can be found first in my book Les Hommes et Les Astres (1960, page 59); then in my three page protocol sent on March 4, 1967, to the chairman of the Belgian Para Comite; then, again, in my recent six page protocol sent on April 28, 1981, to the French Para Comite (I strongly wanted to send the same written and specified hypothesis to Kurtz before the beginning of the American test, but he did not give me the time to do so). This specified hypothesis says: The Mars effect is vindicated if a significant excess of Mars in the key sectors rise and culmination added is found at the birth of the sports champions. According to this hypothesis, the statistical replication of the Mars effect by the Belgian Para Comite is obvious. On the total of 535 births of athletes, 119 were born when Mars was in key sector 1 (rise) or in key sector 4 (culmination); expected frequencies for these sectors 91.7; difference between observed and expected frequencies +27.3, excess significant at the .01 level. Actually, among the 535 athletes of the Para Comite's sample, 22.2 percent were born with Mars in key sectors, which is a percentage superior to the percentage I found in my own original sample where Mars is in key sector for only 21.4 percent of the cases. Note that the Para Comite did not try to evade the fact that they did replicate the Mars effect (except that they did not apply the right statistical treatment of the data I proposed in my written protocol to them). Those who are claiming that the replication of the Para Comite is only a partial one because the result of the key sector of the rise is better than the result of the key sector of the culmination are (i) not well informed about the specified hypothesis; or (ii) are making a statistical mistake (all trained statisticians will agree that, in a relatively small sample of 535 cases, it should happen that one key sector will give a better result than the other! The fact is that, in the Para Comite sample, both key sectors present an obvious excess of Mars frequency); or (iii) are looking for a loophole in order to put some confusion in a matter where there is nothing but a clear success for the Mars effect

7. Several people were puzzled that I objected to the use of basketball players in the American test. My statement was misunderstood. Let me clarify this point. I never claimed that top basketball players should not be included in any test of the Mars effect. Concerning the American test, I did not object to the inclusion of the basketball players listed in the Lincoln Library of Sports Champions because they all seem well-known. In the protocol for verifying the Mars effect I sent in 1967 to the Belgian Para Comite and in the protocol I sent recently (April 28, 1981) to the French Committee, I made no mention of exclusion of top basketball players. My last experiment on 435 new champions also includes some outstanding basketball players (Series C, Volume 6, 1979).

So what happened? Let me tell the story. It is Kurtz himself who pointed out to me at our meeting on July 1977 in Buffalo, that, in my original sample, basketball shows the lowest Mars effect among other sports specialities. I was aware of that, of course, and I suggested to Kurtz that it would be preferable to avoid basketball in case of a new test in USA. This would give a better chance to successfully replicate the Mars effect. But Kurtz did exactly the contrary. Without warning me, he chose for his test a whole Who's Who in Basketball in which he made no selection at all among the thousand players listed. I objected to this procedure, and I still object, but that is all. By the way, the reader is now in a position to appreciate how improbable is the claim repeatedly made by Kurtz-Zelen-Abell that "before we began our research, Gauquelin agreed upon the use of the Who's Who in Basketball" (The Skeptical Inquirer, Summer 1980, page 62). I am, after all, not a masochist!

8. Our work on personality and planets has been published in all details in several volumes by our laboratory (Series C, Volumes 2-3-4-5 and Series D, Volumes 1-4-7-8). Our results are also available in a more popular form in some of my books like Cosmic Influences on Human Behavior or The Spheres of Destiny.
9. Gauquelin M., Gauquelin F., Eysenck S. B. G. (1979), "Personality and Position of the Planets at Birth, An Empirical Study, "Brit. J. Soc. and Clin. Psychol., 18, 71-75.
10. Gauquelin M., Gauquelin F., Eysenck S. B. G. (1981), "Eysenck's personality Analysis and Position of the Planets at Birth: a Replication on American Subjects," Person. & Ind. Diff., 2, 4.

COMMENTS BY H.J. EYSENCK:

Dr. D. Nias and myself, in our book on Astrology - Science or Superstition?, to be published early in 1982 by Maurice Temple Smith in London, have devoted a whole chapter to an examination of Gauquelin's contribution, and particularly including a discussion of the Mars effect, and the debate concerning it. Having gone into the matter fairly carefully, we have come to much the same conclusion as Mr. Curry; we have no doubt that the only people to emerge from this rather vicious debate with scientific credit are the Gauquelins and Professor Rawlins, and that the CSICOP has handled the whole affair in a manner that cannot really be defended on rational grounds. Curry's very detailed treatment should now put an end to this whole discussion; anyone interested in coming to an independent conclusion will find all the necessary facts in his paper.

There are one or two points which it may be worthwhile commenting upon from the point of view of a recent survey of the whole literature on "sport and personality" written by myself, Dr. D. Nias and Dr. D. Cox, and about to be published in Advances in Behaviour Research & Therapy in the Spring of 1982. The general conclusions arrived at in this monograph are relevant to several points in the discussion, and readers interested in the debate are advised to consult our very detailed monograph in order to decide for themselves how the arguments presented by Gauquelin and his opponents stand up to confrontation with a large body of empirical evidence on the relationship between excellence in sport and personality.

Let us first consider the point concerning the use of basketball players in the calculations offered by the CSICOP. Gauquelin had noticed in his European sample that basketball players had a very low Mars effect, and the fact that the same was found in the American sample is really a finding that may be regarded as a replication of Gauquelin's earlier experience. Taken as such it cannot be used to criticise or deny the existence of the Mars effect in general. We have found ample evidence in our work that different types of sport require different types of personality, and even in a single sport, such as shooting, we have found that the precise nature of the task makes a very great difference in the type of personality best fitted for the task. Thus extraverts are best at types of shooting which require sudden, explosive action, whereas introverts are better at types of shooting where the shooter has ample time to make preparation, and does not have to respond to sudden emergencies. There is no reason to assume that the Mars effect, assuming that it is somehow related to personality, must apply to all sports; if experience shows that baseball players are not covered, then they should not be included in future tests of the Mars effect.

Such a decision, of course, should be followed by further research. We have noted in our monograph that there may be important differences in personality between sportsmen taking part in individual sports, and those taking part in team sports; possibly team sports altogether do not show the Mars effect to the same extent as individual sports. Such an hypothesis is testable, and could provide the beginnings of a more theoretical approach to the whole problem of the relationship between sport and planetary position. On this point, therefore, we agree with Gauquelin and feel that in the American sample basketball players should not have been included.

On another point, Gauquelin states that "highly successful champions very often possess what we described as the 'Mars temperament.' Such temperament is not absent in less renowned athletes, but it is less marked and not more frequent than in non-athletic people." This may sound, as Curry says, "like a simply common-sensical conjecture," but we do not find very much evidence for it in our monograph. Sometimes differences are found between outstanding athletes and average athletes, but these tend to be more in relation to stability than extraversion. The results summarised by us do not disprove Gauquelin's conjecture, but the evidence in favour of it is relatively weak, possibly because not very much effort has been devoted to a resolution of this problem. Clearly the answer to the question raised by Gauquelin must be found in further research on the personality of sportsmen, along the lines we have discussed in our monograph. However, what is clear from our own work is that less successful sportsmen have a temperament differing from the non-sporting majority in the same direction as does the temperament of outstanding sportsmen, and consequently it is odd that not even a small Mars effect is noticeable for them. This presents a difficulty for any theory of the Mars effect, but of course it does not deny the validity of the effect itself, as applied to outstanding sportsmen.

Last but not least, I would like to express my agreement with Curry's view "that it is high time that Gauquelin's rather more interesting and fruitful research in the field of 'planetary temperaments,' i.e. personality receive consideration." A recent paper by M. Gauquelin, F. Gauquelin and S.B.G. eysenck, entitled "Eysenck's Personality Analysis and Position of the Planets at Birth: A replication on American subjects" is due to be published in Personality & Individual Differences, and it was found that "the results of this study on American data are in very good agreement with those of a similar study previously carried out by the authors on European data. A correlation between Eysenck's personality dimensions and the position of the planets at birth was again found. Extraverts are significantly more frequently born when Mars and Jupiter had just risen or had just passed their upper culmination; introverts when Saturn had just risen or just passed its upper culmination. Mars and Jupiter appear to be also associated with psychoticism and Saturn associated with non-psychoticism. Again no positive effects were found for neuroticism." Clearly personality data and relations are replicable, and are not subject to the same kinds of difficulties as may attend the definition of "outstanding sportsmen." Whether the observed relations can only be found in famous or outstanding people, or are to be found also in the average man and woman, is one of the most interesting research topics thrown up by the original and creative work done by the Gauquelins in this field.

COMMENTS BY H. KRIPS:

What does the scientific establishment do when threatened by an intruder - particularly one (like Astrology) which rises from a 17th Century grave? Patrick Curry has given us some insight into this, in his alarming tale of "Research into the Mars Effect." And this same story is unfolded, in even more grisly detail, in Dennis Rawlins' "Starbaby." What can one make of it all? Can the villains really be as black as all that, can the heroes really be so simon pure? Is there really a scientific mafia, suppressing the French connection? Will Richard Nixon make a comeback?

But there is a serious side to these issues. It is tempting to get too carried away with the sociological issues - to voice platitudes about establishments under threat, and think that's all there is to the matter. As Curry points out, there are additional issues of substance : just what is the evidence, does it support the Gauquelins' theory, how "good" is the Gauquelins' theory qua scientific theory (and, even more basically, just what is the Gauquelins' theory - and by that I don't mean just those isolated consequences which have attracted the attention of the Z.K.A. putsch). Curry discusses the first two of these issues; and his verdict seems by and large to be in favour of the Gauquelins' theory. But this is only the beginning of the story. For a theory to get evidential support is, after all, only a first step - at best is is a necessary condition for rational acceptance. Indeed, if one believes the philosopher of science Imre Lakatos (see Lakatos, I., 1972), it is not even that : according to Lakatos every scientific theory worth its salt is born floating in a veritable "sea of anomalies." It is the explanatory power, fertility, etc. of the whole "research program" of which a theory is a part, which convinces scientists to set aside the unfavourable evidence, and develop the theory further. How then does the Gauquelins' theory fare when assessed in terms of the Lakatosian view? Not all that well, although perhaps not any more badly than some other more notable theories. A particular lack in the Gauquelins' theory is the absence of a satisfactory mechanism to

explain the "Mars effect" and the other correlations which they have observed. Without such a mechanism, the theory clearly lacks importantly in explanatory power, and also lacks the power to suggest what needs changing when empirical fit becomes a problem. Suggestions for mechanisms are of course made in the Gauquelins' books, but none of these stand up to critical scrutiny (see Krips, H., 1979). This same deficiency however, dogged Darwin's theory of evolution at its inception : an account of the mechanism for "passing on" survival traits was a glaring omission from his original theory, and was accepted as such by Darwin and his apologists. Nevertheless the Darwinian program persisted - perhaps because of its superiority on other counts (or does one construct a pro-Darwinian mafia to explain its acceptance?)

Perhaps inevitably, this resort to philosophy of science does not resolve the most burning questions : in particular, from a Lakatosian viewpoint, it is not clear, one way or the other, whether the Gauquelin theory ought to be accepted (or even be considered worthy of serious investigation). But what this consideration of philosophy of science does, is to make one refocus on different issues as being of significance. The question of whether or not Gauquelin's theory is "supported by the evidence" becomes of less importance, than the question of whether it generates a research program which has explanatory power, is fertile, etc. - and these issues in turn focus one's attention on the question of what the mechanism for the "Mars effect" is. My feeling is, that less effort spent on statistical investigations, and more on theoretical research, might be the Gauquelins' best answer to their critics.

Finally, let me say what is one of the interesting points for a philosopher of science, to emerge from the Gauquelin - Z.K.A. controversy. It illustrates perfectly a claim which Popper already made in the 1930's (Popper, K., 1968), viz that there is a degree of arbitrariness involved in even the most sacred of scientific cows - the rite of deciding whether or not a theory "fits the evidence." To cite just two instances : it is to some extent arbitrary which "level of significance" we adopt in statistical testing, and what sampling procedures we use. Popper felt that this undesirable intrusion of arbitrariness into the scientific process, could be excised by obtaining agreement between the disputants over some hypothesis, about what would count as favourable or unfavourable evidence, prior to a test actually being carried out. But, as the history of science in general (and the Z.K.A.-Gauquelin controversy in particular) has shown, this policy turns out to be a pious hope - it is a rule followed more in the breach than in the observance. What then are we to say? Do we castigate Z.K.A. or the Gauquelins for failing to come up to Popperian standards, which few, if any, other scientists follow in practice? Or do we rather accept that there's something wrong with the Popperian standards? I follow Lakatos, in opting for the second alternative. More particularly, I think that Lakatos's view of science, makes us see the role of questions of "evidential fit" in something more like their proper perspective, i.e. as having secondary import next to questions of explanatory power, fertility, etc.

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COMMENTS BY I.J. GOOD: "IS THE MARS EFFECT AN ARTIFACT?"

"The chances of anything coming from Mars are a million to one," he said. "The chances of anything coming from Mars are a million to one -- but still they come."

-- Jeff Wayne's musical version of
The War of the Worlds.

Introduction. Michel and Francoise Gauquelin do not believe in classical astrology but they might have discovered some statistical "cosmic influences" that some people call "neo-astrology". The most discussed example is the "Mars effect". The Editor of ZS has invited my comments on this topic and I am responding, but I have not had time to review all the relevant literature. It seems to me, however, that no firm conclusions are yet possible.

The basic observation of Gauquelin was that of 2088 European sports champions, of whom 452 were born when Mars was in Gauquelin's sectors 1 or 4, that is, there were 452 "successes". This sample includes 535 Belgian sportsmen selected by the skeptical Belgian Committee for the Scientific Study of Paranormal Phenomena. If, as a "null hypothesis", there is no Mars effect, then the expected number of successes would be $17.17\% \times 2088 = 358.5$. This percentage 17.17 was independently calculated astronomically by Gauquelin and by Rawlins. But the Belgian Committee thought that the percentage 17.17 might vary from one area to another and from time to time. Marvin Zelen suggested that a control sample of ordinary people should be found, born at the same place and on the same day as a champion. This was extended to "within three days" to make the control sample large enough, namely 16756 people, but these corresponded to only 303 of the original 2088 champions. Expressed as a 2×2 contingency table the result of the Zelen test was

	Effect	Non-effect	Sample Size
Champs	66	237	303
Non-champs	2745	14011	16756
Totals	2811	14248	17059

The figures 66 and 2745 can be obtained from Gauquelin (1977, pp. 31 and 34). (See also Kurtz, Zelen & Abell, 1979/80a; and Rawlins, 1981.) The tail-area probability for this table is .007. Although this would be small enough to reject the null hypothesis in most biological work it is not very impressive when we are considering astrological matters. The sample of 303 champions is too small to give a decisive result, but the work required to obtain a decisive result might be prohibitive.

Note that 17.17% of 16756 is 2877 which is 132 more than the observed number of 2745. If 17.17% is the correct percentage in an "infinite" population, the probability of a deviation as large as 132 is 1/280. This suggests that the Belgian Committee was right in thinking that the percentage of 17.17% was unreliable. On the other hand if the overall percentage of ordinary people who show the Mars effect is less than 17.17, then Gauquelin's

original observation of 452 successes among 2088 champions would be even more striking.

Let us consider the significance of Gauquelin's original observation on the assumption that 17.17 is the correct overall percentage of ordinary people who show the Mars effect, even though that percentage is suspect. I am presenting the argument partly for its possible methodological interest for experiments in parapsychology. Assuming the null hypothesis, the standard deviation is $\sigma = [2088 \times .1717 \times (1 - .1717)]^{1/2} = 17.23$. The bulge, allowing for a minor "continuity correction" is $451.5 - 358.5 = 93 = 5.40\sigma$. The probability of so large a deviation in the right direction is about 1/30,000,000. It is not accurate enough to say "one in millions" because of the "dwindling" that I shall discuss presently. It is even more slapdash to say that the odds are "millions to one against chance".

In this note I shall assume for the most part that the sampling was done correctly. In my opinion Gauquelin is conscientious and intelligent and if there is anything wrong with his work it is subtle. It should not be forgotten that he was a pioneer in his attacks on astrology even if in the end he was sucked in.

How to dwindle a tail-area probability. A tail-area probability of 1/30,000,000 might seem impressive enough to convince any one. But its impact can be dwindled partly by allowing for special selection and partly by using a Bayesian argument. This was done in Good (1930) and here I shall present the argument somewhat differently.

Let p denote the physical probability that a future champion sportsman would be born with the "Mars effect", that is, with Mars in Gauquelin's sector 1 or 4. Let H_0 denote the null hypothesis that $p = .1717$. Let's take as the rival hypothesis H_1 the assumption that p has a uniform prior density between 0.1 and 0.3. (The final odds would only be halved if we took 0.5 in place of 0.3 here and this suggests that the "Bayesian robustness is adequate.") Given these assumptions we can work out the "Bayes factor" in favor of H_1 , that is, the ratio of the final (posterior) odds of H_1 to its initial (prior) odds. (Jeffreys, 1938; Good, 1950. "Odds" means $p/(1 - p)$ where p is a probability.) I call the logarithm of the Bayes factor the "weight of evidence", a definition that C. S. Peirce (1878) would have used if he had not made an error: see Good (1981). The advantage of using weights of evidence or Bayes factors, rather than final odds or final probabilities, is that Bayes factors are mathematically independent of the prior odds of the null hypothesis. This is an advantage because the prior odds of a hypothesis are liable to be very subjective, that is, very variable from one judge to another, and judged to lie only in a wide interval even by one judge. Although the formulation of H_1 is also subjective its variation from one judge to another is likely not to have much effect on the conclusions reached in the present problem.

It turns out that the Bayes factor in favor of H_1 from the observation of 452 "successes" in 2088 "trials" is about 250,000.¹

We must now allow for the number of attributes that could have been entertained for the people sampled, such as professions, personality, religion, and physical features. Take say 100 for this number. Then pay a factor of say 5 because Gauquelin insisted that the athletes should be outstanding, and a further factor of say 8 for the choice of Mars. I hope no one is going to claim that Mars's being the god of war is of any importance to the argument. If then the initial odds are x that there is some personal attribute associated with some planet in Gauquelin's sectors 1 and 4, then the final odds are of the order of $60x$ because the Bayes factor is about 60. I have not paid a factor for the selection of the sectors 1 and 4 because Gauquelin liked these sectors for other reasons. I have not tried to evaluate these other reasons and in this respect my analysis is incomplete, but life is short and science is long.

If a Bayes factor in favor of some hypothesis, provided by an experiment, turns out to be appreciable, and 60 is certainly appreciable, then the hypothesis must be worth taking seriously provided that the experiment was worth doing in the first place. This further argument for the importance of a Bayes factor was pointed out by Good (1950, p. 70). It is not my purpose to discuss whether the experiment was worth doing.

Can the prior odds be enhanced by de-astrologization? The rational judgement of an initial probability depends on how a hypothesis fits in with one's previous knowledge or preconceptions. If you believe in the existence of ancient Greek gods, in which case you may as well stop reading, then your value of x will be appreciable. To parody Voltaire, although the Greek gods did not exist men invented them. But if you think that the great ancient religions were twaddle, humbug, and balderdash, then you might feel that the best way to enhance x is by de-astrologization. This might be achieved if there is a slightly greater tendency for sports champions to be born at certain hours of the day as compared with non-champions. More precisely it is a matter of the recorded times of birth rather than the actual times, for champions might be more likely to be the sons of fathers who report the times of birth accurately. In fact Dean & Mather (1977, p. 386), quoting Gauquelin, say that professional people are more accurate in their information than "working classes".

Suppose, for example, that 1.6σ of the bulge, for the sample of 2088, was genuinely to be expected for biological and reporting reasons connected with hour of birth, then it would not be difficult, after the dwindling process, to swallow the remaining 3.6σ as being due to chance. As Rawlins (1979/80) says "Mars appears near the sun more often than not", so the sectors that Mars is in must be correlated to some extent with the time of day. I have not seen Gauquelin's raw data so I do not know whether this partial explanation will hold water, and maybe Gauquelin has already taken the possi-

bility sufficiently into account. Note that if there is a biological effect connected with time of day it would not imply that the planets as such have any effect - only that there would be a small correlation with their positions at the time of birth. A similar comment applies to the effect that the season of birth might have.

It will be noticed that my arguments have a subjective element, and in my opinion some use of subjective judgement is inevitable in every application of statistics. Statistics aims to reduce subjectivism and ideally to eliminate it, but this ideal is never entirely achieved in any application that I can think of. One of the methods of trying to decrease subjectivism is to try to obey the usual axioms of subjective or logical probability (for example, Good, 1950, 1976). This opinion is contrary to Curry (1981) who said "Subjectivism is, of course, avoidable ...". His use of the expression "of course" suggests that he was not aware of a growing neo-Bayesian subjectivistic school of statistics, unless he thought the adherents of this school are stupid.

The U. S. test. An independent test was carried out in the U. S. by Kurz, Zelen, and Abell and it is discussed in the Skeptical Enquirer, 2, no. 2, in four parts: Kurz, Zelen & Abell (1979/80a, b), Rawlins (1979/80), and M. & F. Gauquelin (1979/80). The Gauquelin's reached conclusions opposite to those of Kurz, Zelen & Abell because there was dispute about which of the sportsmen were outstanding. The sample was small and its only really curious property was a strong tendency of the less good sportsmen not to show the Mars effect. In some of the relevant literature there is discussion of whether a tail-area probability is slightly less or slightly greater than 0.5. I found this aspect uninteresting for a topic so far out as the Mars effect.

Provisional Conclusions. The Mars effect may be real but it might be partially explicable by the diurnal times of birth and by the less accurate reporting by the fathers of non-champions. If only a third of the bulge of 5.4σ can be explained away in this manner, then, even if the 17.17% were correct for ordinary people, the remainder of the bulge would no longer be startling enough to merit the attention of those who regard the ancient Greek religion as twaddle, humbug, and balderdash (whatever its merits for literature may be).

The two samples of ordinary people showed the Mars effect with percentages significantly lower than 17.17. If the overall percentage is in fact much below 17.17 then the evidence for the Mars effect would be greatly increased, but the fact that the percentage of 17.17 is unreliable undermines all the evidence, except for the evidence from the Zelen test which supported Gauquelin's thesis inconclusively.

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COMMENTS BY PIET HEIN HOEBENS:

In his admirable attempt to sum up the complicated "Mars Effect" controversy, Mr. Curry has addressed two crucial questions: 1) Has the affair affected the credibility of CSICOP?; 2) Is there such a thing as a "Mars Effect"?

Ad 1): The affair has been variously described as "the biggest scandal in the history of rationalism" and "a storm in a tea cup." One of the complicating factors in the present debate has been a tendency on both sides to exaggerate the importance/triviality of the issue. From an initially neutral position I have, for the past ten or eleven months, conducted some investigations of my own. This had resulted in a "Mars Effect" file containing several hundred items. My inquiries have not yet been completed, and I do not wish to seem discourteous to some of my correspondents by committing myself to a final verdict at this stage.

However, I see no reason to dissimulate that I am most unhappy about the manner in which CSICOP has handled the controversy to this point. My Oct. 9 letter to Professor Kurtz, in which I expressed my misgivings, has been widely circulated. The ensuing correspondence with several supporters of the Committee has alas failed to provide me with a convincing argument against Mr. Curry's (and Mr. Rawlins') conclusion that CSICOP's involvement in the testing of M. Gauquelin's claims should serve as a warning rather than as an example. No doubt there are some extenuating circumstances, but it is incumbent on the leaders of the Committee rather than on me to bring these to the attention of ZS readers.

My private guess is that the root of the trouble may be in the philosophy dominant among the present CSICOP leadership. If my guess is correct, the authors of the KZA reports may initially have taken it for granted that a sceptical investigation of any "paranormal" claim would automatically result in a swift and unambiguous confirmation of sceptical predictions. When the "Mars Effect" failed to oblige, they were taken by surprise and had to improvise a strategy to protect scepticism from premature "falsification."

The Committee has often been criticized for the wrong reasons. This time, however, there is a real credibility problem. If CSICOP wishes to be true to its stated objectives, some re-thinking (and some re-structuring) seems urgently required.

Ad 2) Blessed with strong aesthetic prejudices against "cosmic vibrations" and other Blavatskian concepts, I trust that a non-occult explanation will eventually be found for the data suggestive of a "Mars Effect." Perhaps an extremely subtle artifact is involved. However, at this stage the sceptic should be prepared to acknowledge that the Gauquelins have discovered a legitimate anomaly. Whether this anomaly will later turn out to be of profound significance for the history of science I do not know.

In the concluding paragraphs of his paper, Mr. Curry urges the reader to start thinking about how a genuine planetary effect might come about. I will respond by offering a half-baked speculation.

Given the enormous number of "events" in our universe, incredible coincidences are bound to arise by chance alone. Feed an advanced computer

with all "data" about the cosmos, allow the machine a century to search for correlations and a rich harvest of fantastically "significant" yet entirely meaningless "effects" is assured. M. Gauquelin's planetary effects could, in principle, belong in that category. The only trouble with this view is that the Gauquelins are no computers. How then could they ever have hit on those coincidences? Good old ESP might provide the answer. Why not explain the Gauquelin findings as the result of an act of precognitive clairvoyance? In that case, the neo-astrological "effects" would be chance events. The only "paranormal" occurrence would be their detection. Exit Mars. Enter Psi. This solution may hold some attraction for those who do not share the present author's reluctance to believe in such a thing as ESP.

COMMENTS BY LUC DE MARRE:

As I was strongly involved in the work of the Belgian "Para Committee," regarding the so-called "Mars effect" of Mr. Gauquelin, I'd like to bring - especially for the esteemed readers of the Zetetic Scholar - my witness in this matter.

Having collected nearly all of Gauquelin's material - as far as Belgian sports champions are concerned - I have been a member of the committee since the very beginning of the test.

First, I must state that there has never been any dispute as to the material of the test, nor as to the selection of cases. There is an obvious reason for this: prior to any checking, the committee firmly agreed with Mr. Gauquelin about a definite list of 535 specified champions.

But the committee, composed exclusively of astronomers and mathematicians, most of which had read - if not studied - privately, some of the books of Mr. Gauquelin, was very sceptical concerning the results claimed by him. It therefore had a strong suspicion, that either the calculation of the Mars position, or the statistical formulae which he had used, had been - consciously or unconsciously - manipulated.

The first work of the committee therefore was to do the whole of the calculations over again. A big computer was programmed for this purpose.

As a matter of fact, the committee was unable to discover any mistake or error in Mr. Gauquelin's calculations nor in the results which he claimed. Indeed, the same anomalies (significant peaks in key sectors: chiefly in the rising, but also in the culmination sectors) were established by the committee. From then on, it could no longer deny that Mr. Gauquelin had scored, once more, with the list of 535 champions.

Having reached this critical point, and taking into account that the actual results of the 535-test were a confirmation (and even a small improvement!) of Mr. Gauquelin's previous results, the committee normally should have had to agree upon the existence of a Mars effect, at least in the sample of the 535 champions.

But it remained very far from doing so. Obviously, an acceptance of this Mars effect should have obliged these people to revise part of their scientific and even philosophical bias.

The committee, indeed, since its foundation in 1948 and in spite of its name and its purpose, did never acknowledge any claim beyond the frame of traditional, established, official science. Was it driven by that old sophism: "It may not be, so it can't be"?

On the other hand, it would be unfair, to accuse the committee of laziness. In the course of the following years, it undertook a great number of counter-experiments. This tremendous work had no other aim than to establish, that the surplus in key sectors was due to anything else but a Mars influence.

However, the results of all these counter-experiments tended to confirm Mr. Gauquelin's hypothesis. In particular, a sliding of the birth hours, in function of the alphabetical order of the champions, showed beyond all dispute that Gauquelin's theoretical (expected) frequencies were correct.

In September 1976 the committee published a 17-page report on its work concerning the research. It was astonishing, to see it did not mention any of these counter-experiments; on the contrary, it accused Mr. Gauquelin of imaginary demographic errors. This latter item was the more surprising as it was Mr. Gauquelin himself who had informed Mr. Dommanget, member of the committee, about the existence of a demographic problem which had to be solved, as well as about the means to achieve that solution.

The committee also did not take into account any criticism of its work, not even when serious remarks about the procedures were uttered by such eminent people as prof. Chauvin of the Sorbonne-University in Paris or prof. Baillaud of Clermont-Ferrand.

I have always been a patient and tolerant man. But after the report, referred to above, had been published in the way it was, I felt morally compelled to resign from the Para Committee.

This painful experience did not bring me, however, to a negative judgment about all men of science. A few years later, when Mr. Gauquelin again asked me to collect the necessary material for a new test, this time in the U.S.A. (the so-called Zelen test), I spent days and days in the registries to gather hundreds of birth records. This is why I am really shocked and deceived in seeing that, in fact the same thing happened with the CSICOP, as with the Para Committee: a distortion of truth to save, cost what it may, the interests of anti-astrology.

I remember a sentence, which I once read in The Devil's Dictionary of Ambrose Bierce: "Prejudice is a vagrant opinion, without visible means of support."

COMMENTS BY J. DOMMANGET:

a) After several years of various experimental and theoretical research and very careful examination of the above mentioned effect claimed by M. M.GAUQUELIN, the Belgian Committee "PARA" has clearly expressed its views on the subject in the issue n° 43 of its NOUVELLES BREVES (September 1976).

Afterwards unfortunately, unfruitful discussions between various people and groups of people interested in this problem have quite completely darkened any clear understanding of this problem. This was due to the fact that none of the interested people - M. M.GAUQUELIN included - seems to have been aware of the imperious necessity to adopt first of all, by a common agreement, a correct analysis of the fundamental mechanism which generates the observed distribution diagramme for the planetary "classes" considered by M. M.GAUQUELIN. Such an analysis - as the one given by the Belgian Committee in the above mentioned publication and which seems to be at least apparently systematically ignored - would have avoided many misunderstandings.

For the Belgian Committee, it appears that it is now time to clearly reaffirm once more its well-established position since 1976 in order finally to make a firm proposal to facilitate any further discussions and to save a lot of precious time.

b) The position of the Belgian Committee is recalled hereafter in its original French formulation (pp. 342-343 of issue n° 43 of the NOUVELLES BREVES):

"Après étude et vérification des travaux de M. M.GAUQUELIN, le Comité :

- 1) reconnaît que le calcul des classes dans lesquelles apparaissent les instants de naissance des individus concernés, a été effectué correctement par M. M.GAUQUELIN;
- 2) déclare qu'en utilisant un nouvel échantillon de 535 sportifs, le diagramme de fréquence observé en classes présente bien l'allure générale trouvée précédemment par M. M.GAUQUELIN pour d'autres échantillons;
- 3) note que les calculs des diagrammes de fréquences observées, ceux des diagrammes de fréquences théoriques par les méthodes proposées par l'auteur et ceux des tests du χ^2 ne paraissent contenir aucune erreur.

Par contre, le Comité conteste la validité des diverses formules adoptées par M. M.GAUQUELIN pour le calcul des fréquences théoriques car :

- 4) elles ne tiennent pas compte correctement de la probabilité théorique d'arrivée des configurations C_k ;
- 5) elles ne permettent pas de tenir compte de l'éventuelle évolution de la courbe nyctémérale avec le temps;

6) elles font appel essentiellement à l'échantillon lui-même; ce qui, en général, a une influence sur le nombre de degrés de liberté.

Le Comité ne peut donc accepter les conclusions de M. M.GAUQUELIN aussi longtemps qu'elles seront basées sur les méthodes et formules que celui-ci préconise.

De son côté, le Comité propose, sur la base d'une démonstration rigoureuse, la seule formule valable à son avis et explicitant tous les aspects du problème posé."

c) This clearly says that :

1.- When considering different samples of sportsmen, the observed distribution diagramme seems to always present the same pattern (point 2) and IF the "Gauquelin's method" for computing the theoretical diagramme is used the same significant deviation from the observed one, is noticed (point 3);

2.- A complete and correct analysis of the mechanism generating the distribution diagramme leads to a different (and more general) formula than the one adopted by M. M.GAUQUELIN. The "method" proposed by M. M.GAUQUELIN in the computation of the theoretical diagramme appears unsufficiently representative of the phenomenon (points 4 and 5);

d) Consequently, the opinion of the Belgian Committee is that the MARS-EFFECT has not been demonstrated. The Committee regrets that M. M.GAUQUELIN has claimed and continue to claim at any time and at any place that the experiences conducted by the Belgian Committee proves the validity of this MARS-EFFECT but that the Committee does not admit it.

The Committee reminds that it has only shown that the use of the "(erroneous) Gauquelin's method" leads to a significant result. As Tong as the validity of M.Gauquelin's method has not been well established and duly proved, it appears impossible to the Belgian Committee to pursue any further discussion.

e) Therefore, the Belgian Committee "PARA" proposes to all parties engaged in any research on the so-called MARS-EFFECT claimed by M. M. GAUQUELIN :

- to recognize clearly the entire validity of the analysis made by the Committee and published in the NOUVELLES PREVES (by the lack of any other similar analysis),

or

- to indicate without any possible doubt with an appropriate theoretical demonstration if necessary, on which precise point this analysis could appear erroneous.

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It is the opinion of the Belgian Committee "PARA" that without an agreement on a unique and correct analysis of M. M.GAUQUELIN's problem, further discussions will be vain.

MICHEL GAUQUELIN'S COMMENTS ON THE STATEMENT BY THE BELGIAN COMMITTEE
"PARA":

Among the reactions following Patrick Curry's stimulus article on the Mars effect in ZETETIC SCHOLAR, the statement issued by the Belgian Committee Para, written by its president the astronomer J. Dommange, deserves a special treatment, I think. People who are sufficiently aware of the Mars effect controversy can fully appreciate the impudence of this extraordinary statement. For the other readers, I would like to reply. The Belgian Committee Para reluctantly admits to having replicated the Mars effect on a new sample of champions but claims that "the Mars effect has not been demonstrated" because Mr. Gauquelin's theoretical (expected) Mars distribution is "erroneous."

I would like to demonstrate that Dr. Dommange and his Committee are intentionally "forgetting" all the work which was done (and that they know was done) for successfully solving the problem of the theoretical distribution of Mars at the birth of the sports champions.

First of all, the reader has the right to know that Dr. Dommange did not publish in the Committee Para report (NOUVELLES BREVES, 1976) his own expected Mars distribution which is claimed to contradict our own expected Mars distribution. His ZETETIC SCHOLAR text gives the false impression that the Belgian Committee Para did published its own expected distribution for Mars. But it didn't. There is an obvious reason for that. Dommange knows damn well that his expected frequencies can not be different from our own calculations and, consequently, that means he clearly replicated the Mars effect on a new sample of champions.

There is another serious "oversight" in the Committee Para's statement and report which was pointed out by Prof. de Marre in ZETETIC SCHOLAR: the lack of information concerning the counter-experiments carried out by the Committee. Prof. de Marre, former member of the Committee Para, was strongly involved during seven years in the Mars effect experiment. He mentioned the counter-experiments undertaken by the Committee Para: "the results of all these counter-experiments tended to confirm Mr. Gauquelin's hypothesis. In particular, a sliding of the birth hours, in function of the alphabetical order of the champions, showed beyond all dispute that Gauquelin's theoretical (expected) frequencies were correct. In September 1976, the Committee published a 17-page report on its work concerning the research. It was astonishing to see it did not mention any of these counter-experiments." (It will be too long to give the details of this very interesting control. The interested reader can find an account of it, with the main figures, in my article published in the Int. J. of Interdiscipl. Cycle Res., 1972, 3, 3/4, pp. 381-389. I have the full print-out of the data which could be published if necessary.)

Most aggravating, Dommange and his Committee sham by ignoring Dennis Rawlins and George Abell's analyses of the problem of the theoretical distribution of Mars. But, of course, they were very well aware of them.

Denis Rawlins' memorandum was published in PHENOMENA (May 1978) and sent to Dommange in due time. As most people know, Rawlins' memorandum is the analysis of the Mars expected frequencies problem, and the conclusion

of his theoretical demonstration is that "Gauquelin has made fair allowance for the effect under investigation." In his "Starbaby" (FATE, October 1981), sent also to Dommangelet in time, Rawlins is explicit in speaking of the "report and alibi of the Belgian Comite Para which some year earlier, to its surprise, had confirmed the approximate success rate Gauquelin had predicted (for the Mars effect)."

George Abell sent me a letter on May 3, 1980 (with a copy to Paul Kurtz). In that letter he told me he had calculated the theoretical distribution of Mars sectors at the birth of the champions with the help of his collaborator Albert Lee. His conclusion was that he found the same theoretical distribution as ours. I sent Abell's letter to Dommangelet in May 1980 asking for his comments. He did not answer me but wrote directly to his colleague Abell (without a copy of his letter to me). George Abell answered Dommangelet on March 14, 1981 (with a copy of his letter to me). In that letter, Abell is as explicit as possible concerning the problem of the theoretical distribution of Mars. He says: "a student and I simply did the calculation rigorously, as far as the astronomical factors are concerned. For the curve of birth during the day, we used three different samples, as you can read in my letter to Michel (which you say you have), and it made no particular difference. The upshot is that we found Michel's theoretical curve to be substantially correct."

The Zelen test is also amazingly ignored by the Committee Para and its president. I say "amazingly" because the very origin of the test proposed by Marvin Zelen was the following statement published by the Committee Para in THE HUMANIST Jan/Feb 1976 issue, and repeated in its May/June 1976 issue, questioning our methodology. The Committee Para alleged that in our calculations, "1. The secular variability of the diurnal demography was not taken into account in the computations. 2. The probability of appearance of the various configurations in a sector is implicitly admitted equal to a constant, and also that the effect of the secular demography are entirely ignored." Curiously, and contrarily to me, the Committee Para, who should have applauded Zelen's suggestion, said nothing at that time. It is easy to understand why. The Committee was very worried by Zelen's suggestion. It knew too well that the test would kill its alibi for rejecting the Mars effect: the test will have only one possible outcome, that is the vindication of our expected frequencies of Mars in "key sectors" among the non-champions population (17 percent); which actually happened.

Now that the reader is informed about all the work which was done for solving the problem of the theoretical distribution of Mars among champions, he can fully appreciate how short Dommangelet's memory is and how extraordinary (May I say outrageous?) the last proposal of the Committee Para in its ZETETIC SCHOLAR statement is. Let me quote it for our intellectual pleasure:

"Therefore, the Belgian Committee "Para" proposes to all parties engaged in any research on the so-called Mars-Effect claimed by M.M. Gauquelin:

- to recognize clearly the entire validity of the analysis made by the Committee and published in the NOUVELLES BREVES (by the lack of any other similar analysis), or
- to indicate without any possible doubt with an appropriate theoretical demonstration if necessary, on which precise point

this analysis could appear erroneous."

We can especially appreciate the sentence concerning "the lack of any other similar analysis," I think. Apparently, Dommangeat considers Abell, Rawlins and Zelen's analyses to be nonexistent. It is not very kind of him! What do these people think about Dommangeat's opinion of them?

Anyway, before taking into consideration the last conceited proposal of the Belgian Committee Para, we have the right to demand that Dommangeat will comply with the following urgent requests:

1. To publish (at last) his own theoretical (expected) distribution of Mars in sectors at the birth of athletes (heavens, he never did so!);
2. To publish (at last) the outcomes of the crucial counter-experiment he undertook (mentionned by Prof. de Marré) which demonstrates the accuracy of our own calculations;
3. to demonstrate on which precise point Rawlins' analysis and Abell's analysis of the problem - which both are in agreement with our analysis - are "erroneous";
4. to explain how 16,000 non-champions born in the same place and on the same week as the champions display a Mars effect in "key sectors" of only 17 percent instead of 22 percent for the champions (results from the Zelen test which shows that the Mars effect among champions can not be due to an astronomical or demographic artifact).

We will be curious and happy to see Dommangeat answer these four points in ZETETIC SCHOLAR. On the other hand, the lack of any answer from him should be interpreted as the impossibility for the Belgian Committee Para any longer to defend its indefensible scientific position.



REPLIES TO HIS COMMENTATORS

PATRICK CURRY

"I don't know if I'm standing on my head or my heels!"

"Sift the evidence. Which end of you is nearer the ceiling?" said Lord Incenham.

P. G. Wodehouse

"Research on the Mars Effect" was written six months ago. In what follows I would like to respond to the comments (direct and indirect) it has since received; on the significance of some missing comments; on a few other recent developments; and conclude with some points of my own.

DIRECT COMMENTS

I have nothing to add to the comments of Dr. Gauquelin, Prof. Eysenck, or Prof. Krips. They enlarge on some points I could only touch on, as well as raising a few new ones worth considering.

The two central issues confronting us here are: (1) the question of scientific impropriety, and (2) the scientific status of the Mars effect (and by implication, the other findings of Gauquelin).

Prof. Good's paper does not address the former, but it does have an important possible bearing on the interpretation of Gauquelin's results. In response to his conjecture, I am neither unaware of neo-Bayesianism nor think its adherents stupid. But I do have many misgivings about this approach, or at least, Prof. Good's use of it.

To begin with, he clearly already has strong feelings about astrology, and a fortiori Gauquelin's results. Gauquelin has been "sucked in" by astrology; the "great ancient religions" and gods are not only twaddle and humbug, but balderdash. In an objectivist approach, which draws on a distinction between "context of discovery" and "context of justification," and at least asymptotically approaches elimination of the effects of a priori opinions in the latter, such strong views would matter little. (That is, unless they led to the abuse of scientific method -- something that can be checked much more easily than opinions.) But they do not bode well in a method based crucially on a priori "probabilities" and "likelihoods."

For example, Prof. Good gives a certain factor to the choice of Mars, adding "I hope no one is going to claim that Mars's being the god of war is of any importance to the argument." But he is in error -- precisely this point is of considerable importance!

Let me remind the reader that the empirical correlation is between (certain positions of) Mars and the births of (i) leading sports champions, and (ii) persons with high extraversion (and to some extent psychoticism). But this is not "a fact without a theory." On the contrary, it is just this correlation (and not one between, say, Venus or Saturn

and aggressive extraverts) that is predicted by astrological theory -- and, I need hardly add, by no other theory.

(Those who doubt that astrology can be construed as a theory, or that historical evidence supports this construal, please see Curry (1981) and Startup (1982).)

Given this situation, Prof. Good's hope turns out to be more pious than plausible -- roughly akin to hoping no one will claim that the (Newtonian) irregularities of Mercury's orbit being uniquely predicted by Einstein's theory is of any importance in evaluating the latter.

Then there is the unsettling subjectivity of taking "say 100" and "say 5" and "say 8" (though I recognize that to some extent this looseness is controlled by the data).

Finally, his attempted "de-astrologization" of the results via the "partial explantion" of Mars' proximity to the sun, and therefore correlation with the time of day, will not hold water. It was recognized and controlled for by both Gauquelin and Rawlins. (The matter of 17% as the expected figure will be discussed below.)

All this -- plus accepting the outcome of the Zelen test ("supported Gauquelin's thesis inconclusively") and the propriety of the U.S. test sample at Prof. Kurtz' word -- completely undermines Prof. Good's "provisional conclusions" that the Mars effect can be explained away as an artifact, and is therefore of little significance.

INDIRECT COMMENTS

My paper elicited statements by Dr. Dommangeat (for the Belgian Comite Para) and Prof. de Marre. A response from Mr. Gauquelin to the former is appended thereto. Given his familiarity with the Comite, and Prof. de Marre's longstanding participation in it, these three papers should be carefully considered in conjunction.

Doing so casts Dr. Dommangeat's statement in an unflattering light. It states that Gauquelin's method (for generating a theoretical distribution of Mars) is "erroneous," that a correct analysis leads to a "different (and more general) formula," and that this conclusion is somehow confirmed by "the lack of any other similiar analysis." But all these claims are flatly contradicted by (and the last claim by the very existence of) (1) Rawlins' 1978 analysis, (2) Prof. Abell's 1980 analysis, (3) the Zelen test outcome, and (4) most remarkably, the Comite Para's own research! Points (1) through (3) are covered in my first paper. For point (4), see Prof. de Marre's letter; and I have myself seen copies of the Comite's control tests, which arrive at the same expected frequency distribution used by Gauquelin.

Furthermore, Dr. Dommangeat was sent copies of all the relevant document covering these points well before issuing this highly misleading statement.

Personally, I think that this constitutes ample evidence that the Comite Belge pour l'Investigation Scientifique des Phenomenes reputes Paranormaux cannot be entrusted with any such investigation. It is very worrying to find this to be the case with yet another such committee (please

see below).

VERY INDIRECT (AND MISSING) COMMENTS

In case they have not already so gathered, readers should be apprised that at the time of writing, not one comment has been received from any member of the CSICOP.* That includes those at the center of this matter, Prof.'s Kurtz (especially), Abell and Zelen, as well as others not too distant -- Prof. Hyman, M. Gardner, J. Randi, and K. Frazier. This, despite receiving copies of my paper and/ or repeated invitations to comment, clarify or rebut -- and half a year in which to do so.

I am personally willing to draw the obvious conclusion -- the Committee has no answers, and the conclusions I reached in July stand unrefuted.

One document did (indirectly) reach me -- "The Status of the Mars Effect," by Kurtz, Abell and Zelen, dated Oct. 15, 1981. Despite the fact that he feels at liberty to quote from the letters of others (even when left unsent by their author), Prof. Kurtz has expressed sensitivity about being quoted himself. Fortunately, there is no need to do so -- this document categorically contains no new points, and contents itself with reiterating the same half-truths, untruths and inconsistencies with which we by now are familiar.

OTHER DEVELOPMENTS

STARBABY

I suppose most readers will have seen D. Rawlins' article, published in Fate, Oct. 1981. Though written independently of mine, "STARBABY" naturally overlaps to some extent. To that extent, I endorse Rawlins' charges.

As with "Research on the Mars Effect," there has been no public reply from the principals involved. There has been a response by CSICOP councillor P. Klass (who denies, however, that he is speaking for the Committee).

It is fortunately not incumbent on me to try to sort out the charges and countercharges not already discussed in my first paper. Reading Klass' "Crybaby," however, I am struck by two things: the extended amateur character analysis of Rawlins, which seems to be the burden of the essay; and the lack of rebuttal of the substantive charges made by Rawlins. After all, those charges do not turn on whether he was an Associate Editor of CSICOP or merely a member of the editorial board. (Nor is it germane "what you missed in the same issue" of Fate if you read a reprint.) The central charge is that there was a "cover-up." That term, I believe, covers much more than heavy editing (of Rawlins) and long delays (for Gauquelin) in the Skeptical Inquirer. It takes in distortion and misrepresentation of the Zelen test outcome, secret sampling in the U.S. test, the persistent refusal to admit to such errors and/or tactics -- even when they were known to the authors, from referees' and others' reports, before publication -- and "censorship" and banning (evidently without a vote of the Council) of

*The comments by Piet Hein Hoebens came to ZETETIC SCHOLAR after Mr. Curry completed his above reply. Also, Prof. Abell has written me that he has not yet been able to reply to Mr. Curry because of his other time commitments.--Ed.

Rawlins -- who, after all, did virtually all the calculations for the U.S. test, and was the only participant with the necessary astronomical expertise to do so.

There is no reason we should be distracted from these central matters by such purely ad hominem animadversions as the letter of Rawlins (attacking Prof. Truzzi) recently circulated by Kurtz.

In other widely-circulated correspondence, I am glad to read P. H. Hoebens' recognition that Rawlins' charges constitute "a challenge that cannot be dismissed as a minor irritation." Prof. McConnell evidently agrees, despite finding this to be "an 'incredibly hilarious' affair." I am glad he and M. Gardner (putatively quoted by McConnell) are able to find humour in what appears to me as a dismal story of scientific malpractice and mendacity. But perhaps I am still insufficiently cynical to be able to join in.

THE CFEPP.

In my July analysis, I mentioned a committee of French scientists who were said to be considering attempting an independent replication of the Mars effect. This is the Comite Francais pur l'Etude des Phenomenes Paranormaux (CFEPP).

I am very sorry to say that at present the situation does not look promising for such a replication. Despite the fact that they have undertaken no such studies for some time, members of the CFEPP -- e.g., Prof. Schatzman, M. Rouze and P. Cousin -- seem reluctant to answer Gauquelin's correspondence except after month-long delays; they apparently continue to express reservations about the long-suffering 17% theoretical frequency (see above, under Comite Para); and perhaps most worrying of all, they seem unable to commit themselves to a precise research protocol agreement with Gauquelin.

Of course, perhaps such foot-dragging is encouraging, in that it implies a very careful and detailed study on their part of how to avoid the errors made by the CSICOP and the Comite Para. Eventually, however, a different and less charitable explanation must appear more plausible.

CORRELATION.

This note is simply to advise readers of a new journal -- "Correlation: Journal of Research into Astrology." I will declare an interest -- I am a Consulting Editor, since this journal publishes any results relating to this field -- negative or positive -- that are competently arrived at; as well as informed speculation. The address for MSS is: S.T. Best (ed.), 4 Shaws Cottages, Worplesdon Rd., Surrey GU3 3LD; for subscriptions: Mrs. F. Griffiths, 98 Hayes Rd., Bromley, Kent BR2 9AB (overseas, in sterling -- £4.50 by surface, 6 by air).

FINAL REMARKS

ON THE COMMITTEES.

There is no doubt but that the entire story recounted above will be thoroughly gone over by sociologists of science. That is certainly as it

should be; there is much to learn. But it is ironic that the various Committees, whose principal goal was the defense of rationalism (against irrationalism), will have provided, by their behavior, such excellent material for extreme sociologists of science. That behavior was engaging in (to quote Prof. de Marre; emphasis added) "distortion of the truth to save, cost what it may, the interests of anti-astrology" -- which accords nicely with the view that scientific knowledge is mainly, even purely, the product of "external" social and ideological factors.

Few people will agree with this extreme position. Besides being intuitively implausible, it is crippled by the paradox of being applicable to its own conclusions. In any case, the abuse of scientific method does not imply that there is no such thing.

But more generally, social factors undoubtedly are of great importance; it is their particular interaction with "realist" factors that must be carefully studied, here as elsewhere.

What such a study would not do, however, is tell us more about planetary effects and temperaments per se. Here, clearly, the most promising approach is a rigorously objectivist and realist one. (By "promising," I mean most likely to both discover new scientific information, and revise and refine "astrological" knowledge.)

The "interests of anti-astrology" seem to realize this (whether consciously or not), because it is just such an approach to Gauquelin's findings that has been so persistently subverted, and remains under threat.

ON ASTROLOGY AND SCIENCE.

Obviously this is a subject that needs more than a few remarks. But by the same token, its importance to the issues at hand means I must say something.

The usual opinion is summarized in a remark approvingly quoted by D. Saklofske in the preceding Zetetic Scholar (No. 8, p. 134): "The two world-views (of science and astrology) are light years apart." It is also succinctly put by P. Thagard (1980, p. 20): "Astrology is our paradigmatic example of a pseudo-science." These are two expressions of the same attitude -- one commonly held with equal tenacity by both scientists and astrologers, neither of whom want their patch "infected" by contact with the other. Both groups therefore have a vested interest in promoting the appearance of an either/or, "your're-with-us-or-your're-against-us" option.

But that dichotomy is a false one. However true it might once have been (which is itself open to debate), the position it represents no longer stands up to critical scrutiny.

For reasons of space, I can present only a skeletal argument here, with references for where to find some flesh.

Replying to Prof. Good, above, I stated that astrology can be legitimately construed as a theory; and that historical scholarship (e.g., Neugebauer (1951) or Cumont (1972)), as distinct from rambling polemic (e.g., Jerome 1977), supports this view.

The question arises, is it a scientific theory, or (preferably) research program? The answer must now be, yes: given that (1) Gauquelin's research is methodologically bona fide research, regardless of whether its results are positive or negative -- a point mentioned and then ignored by Thagard; as it happens, (2) his positive results have been tentatively corroborated by even the hostile Comite Para and CSICOP; and (3) those results not only support the central tenets of astrology -- argued theoretically in Curry (1981) and supported empirically by Gauquelin, F. (1981) and Startup (1981) -- but they are inseparable from traditional astrology, being predicted by it and by no other theory. (NB: Sun-sign columns, etc., are not central!)

The situation, then, is that Gauquelin's findings are both astrological and scientific, in the fullest senses of those words.

I realize that this is a razor's edge, psychologically speaking, which most people will find it easier to fall off of, to one side or the other. Nonetheless, I want to take this opportunity to argue that we should bite the bullet. The only way to do justice to this rather extraordinary situation is to see Gauquelin's findings as the crucial empirical component in a new and promising research program, with very old roots indeed. It is new in the research aspect. It is promising because of its empirical progressiveness (although theoretically underdeveloped, as Prof. Krips noted).

There is one last question I would like to consider. What could legitimately "de-astrologize" Gauquelin's results? One way, of course, would be through the discovery of an important and previously unrecognized artifact. Otherwise, it could only occur through the discovery of a causal chain which thoroughly explains the particular planetary correlations in non-astrological terms.

Assuming for the moment that neither of these developments take place, but empirical corroboration of planetary effects continues...what then? It's just possible that scientists may have to re-consider their common assumption that knowledge of material or efficient causes constitutes an adequate explanation of the phenomenon.

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DEFINING "UFO"

MICHAEL MARTIN

Despite the scientific importance of having a clear definition of "UFO," surprisingly little has been done to produce a definition that is relatively clear and free from problems. For example, the Vallee's maintain that at the present time it is not possible to define "UFO."¹ But this judgement hardly seems justified since so few attempts have been made. The Condon Report did attempt to define "UFO." However, as we shall see, the definition given in the Condon Report was unfortunate. J. Allen Hynek has attempted to define UFO via a definition of UFO Report. However, as we will show, Hynek gives at least two different definitions, and both of these have serious problems. I will argue that although there are serious problems involved in defining UFO, an adequate definition can be given. Let us consider some proposed definitions.

(A) In the Condon Report "UFO" was defined as "the stimulus for a report made by one or more individuals of something seen in the sky (or an object thought to be capable of flight seen when landed on the earth) which the observer could not identify as having an ordinary natural origin, and which seemed to him sufficiently puzzling that he undertook to make a report of it."²

There are several problems with this definition. First, the definition is too broad. Suppose someone is puzzled by something he or she sees in the sky and makes a report. Suppose, further, that shortly after this initial report is made the object is correctly identified by an expert as a weather balloon. The Condon Report definition allows that the object is a UFO despite the fact that it was correctly identified as a weather balloon. Second, the Condon Report definition is not scientifically fruitful. The definition fails to distinguish between cases that can be easily identified by experts and those which remain unidentified after investigation by experts. However, such a distinction is absolutely crucial since only the latter kind of case is of scientific interest.

Moreover, there are several more particular problems with this definition. The reference to not being able to identify the "ordinary natural origin of the object" is troublesome and obscure. Does the "ordinary natural origin" refer to the particular place that the object came from? But if this is what is meant, the definition is too broad in another respect. It is not completely clear what the natural origins of meteors are (are they pieces of a disintegrated planet or what?). But they are not UFOs. Furthermore, the definition is too narrow. Suppose empirical research indicated that UFOs come from Jupiter. Would this be their natural origin? If so, it seems implausible to suppose that once this fact of origin became known, we would no longer be dealing with UFOs, for we still might not know what they were made of, know they traveled from Jupiter, etc.

(B). Hynek in The UFO Experience develops a definition of a UFO indirectly by constructing a series of other definitions in which the definition of UFO report is the most basic. Thus he defines UFOs as the existential correlates, if any, of the UFO phenomenon. He defines UFO

phenomenon as the total class of UFO reports and UFO experiences. He defines UFO experience as the content of a UFO report.³

It should be clear from this that the crucial or fundamental notion in Hynek's account is UFO report. In one place Hynek suggests the following definition:

UFO report - a statement by a person or persons judged responsible and psychologically normal by commonly accepted standards, describing a person's visual or instrumentally aided perception of an object or light in the sky or on the ground and/or its assumed effects, that does not specify any known physical event, object or process or any known psychological event or process.⁴

Hynek's attempt at a definition is in certain crucial respects an improvement over the Condon Report definition. The intent of Hynek's definition is to screen out reports of cranks, as well as reports of objects that can easily be identified as weather balloons, swamp gas, etc. And this is all to the good since the scientifically interesting cases are the cases that remain unidentified after such a screening. However, despite this worthy intent his definition only partly succeeds. The qualifications placed on the person giving the report do indeed eliminate reports of cranks. But the definition, if taken literally, does not rule out reports of objects that can be easily identified by experts. Just because the report does not specify any known physical event or process, etc, it does not mean that some expert who carefully examines the report could not do so. In this definition - despite what he does elsewhere in his book - Hynek does not take seriously the ability of experts to identify objects that competent and reliable lay persons cannot identify.

There is a further serious problem with Hynek's definition. Because of its disjunctive clause, the definition would allow reports of objects that cannot fly - or at least that no one has reported as flying - as UFO reports and this is clearly wrong. What is completely missing in Hynek's definition, and what the Condon Report definition, despite its other problems, at least makes a stab at, is that the object was in the air or, if on the ground, was at least capable of flight.

Elsewhere in his book Hynek gives a rather different definition of UFO:

We can define the UFO simply as the reported perception of an object or light seen in the sky or upon the land the appearance, trajectory and general dynamics and luminescent behavior of which do not suggest a logical, conventional exploration and which is not only mystifying to the original percipients but remains unidentified after close scrutiny of all available evidence by persons who are technically capable of analysing a common sense identification, if one is possible.⁵

There are at least three things wrong with Hynek's second definition. First, Hynek purports to be defining UFO, but actually he seems to be defining a UFO experience. Secondly, the use of the terms "conventional" and "common sense identification" in the definition is unfortunate.

There is no need to explain UFO experience in conventional terms or to identify them in common sense categories. The explanation and concepts of present day science are what are at issue, and these explanations and categories need not be conventional or commonsensical. Thirdly, the definition has a problem similar to his earlier definition of UFO report: an experience of an abominable snowman becomes a UFO experience since it is an experience of an "object upon the land" whose appearance defies present day scientific explanation. But this is an unfortunate implication of the definition.

Clearly there is some room for improvement in Hynek's definition. Attempting to avoid both the mistakes of the Condon Report and Hynek's two definitions I will first define UFO. After this is done I will define UFO report.

A UFO is, of course, an unidentified flying object. This point should not be lost sight of as it was in Hynek's definition. The crucial questions, however, are concerned with identification and they are these:

- (1) What does "unidentified" mean?
- (2) Who fails to make an identification?
- (3) What is the attempt at identification made in terms of?

Let us consider these questions in turn. (1) What does it mean to say that some object X is not identified? First of all, it is important to realize that an object is identified relative to some classification scheme. What may be unidentified relative to one scheme, may be identified relative to another. Suppose one has some definite classification scheme in mind. What does it mean to say that X is not identified relative to that scheme?

On one interpretation, to say that X cannot be identified relative to some scheme would mean that the person using the scheme knows that X does not fit into any of the categories of the scheme. But this sense of "identified" would have certain awkward implications for UFO research. Suppose an object seen in the skies of New Mexico could not be identified as a weather balloon. Then on the present interpretation the object was known not to be a weather balloon; consequently it was not a weather balloon. But suppose that several years later new evidence came to light which identified that the object seen several years before was a weather balloon. Then the object seen in skies over New Mexico was both a weather balloon and not a weather balloon, which is absurd.

A more plausible account of not identifying something is this. To say that X cannot be identified relative to some scheme means that in the light of the evidence available to the person at the time it would be unreasonable to classify the object in terms of the classification he or she is using and quite reasonable for the person to say that the object is unidentified relative to this classification scheme. On this interpretation, one would not be forced into saying that one object was both a weather balloon and not a weather balloon. The correct thing to say would be: Two years ago in the light of the evidence available it was unreasonable to classify what was seen as a weather balloon but in

the light of the present evidence such a classification is reasonable. There is nothing absurd about this. This seems like the correct way to speak about this situation, and consequently we will adopt the second interpretation.

(2) The Condon Report and to a certain extent even Hynek's first definition go wrong in making the lack of identification relative to the person who first makes the report. But this lack of identification must be in terms of competent scientific investigation after detailed and careful investigation. This qualification, as we have seen, has the effect of screening out reports of weather balloons, ball lightning, the planet Venus and so on that competent investigations would quickly recognize.

So, putting this point together with the above analysis of what it means to say that something is unidentified, we get the following:

X is an unidentified flying object relative to all available evidence E, classification scheme S and competent scientific investigators I if and only if in the light of all evidence E available to investigators I it is reasonable for I to assume X is a flying object and that it is not reasonable to assume that X can be classified in terms of scheme S.

The implications of this analysis are perhaps obvious, but it may be worthwhile to point them out explicitly. On this analysis nothing is a UFO in any absolute sense; something is a UFO only relative to some body of evidence, group of scientific investigators and a classification scheme. What may be a UFO relative to all available evidence at one time may be a weather balloon relative to all the available evidence at some other time; what may be a UFO relative to one group of scientific investigators, classification scheme and body of evidence may not be one relative to another group of scientific investigators with more sophisticated techniques of analysis relative to the same body of evidence and a classification scheme. Furthermore, what may be a UFO relative to one classification scheme may not be one relative to a different classification scheme given the same body of evidence and group of investigators.

(3) The question of what classification scheme people usually assume when they say that an object is an unidentified flying object remains. Clearly it is always possible to construct some classification scheme or other in which an object can be identified. One might identify UFO in terms of shape, trajectory, their psychological effect on people who see them and in numerous other ways. Indeed UFO investigators such as Hynek and Vallee have suggested classification schemes for organizing UFO data. But it is clear that the ability of UFO investigators to identify phenomena in terms of these schemes does not mean that UFOs would be identified in the relevant sense. It is not under these classification schemes that UFOs are judged unidentified.

In his second definition Hynek seems to suggest "common sense" as the relevant classification scheme under which UFO cannot be identified. But, as we have seen above, this is much too restricted a view. Scientific classification schemes - schemes that refine and even go beyond

common sense seem much more appropriate to Hynek's purposes. Furthermore, what is considered to be common sense soon changes. The category of "flying saucer" may be much more a part of common sense today than "ball lightning" is. If the category "flying saucer" was part of common sense, an object identified in these terms would necessarily not be a UFO. This seems absurd.

Hynek, in his first definition suggests that the relevant category scheme should be in terms of known physical objects, processes or events or psychological processes or events. But this suggestion seems very unclear and seems to have problems under two plausible interpretations. Let us suppose for the moment that UFO are physical objects and they take up space and have weight. Now if a physical object simply means a familiar physical object, then it may be true now that UFOs are not familiar physical objects. But this can soon change given wide spread UFO flaps and wide publicity. UFOs may become as familiar as airplanes or birds in the sky and people would feel at ease talking about flying saucer shaped objects, flying cigar shaped objects, and so on. But would this mean they were not UFOs? This seems impossible.

On the other hand known physical objects may simply refer to physical objects that have been studied by scientists, that have definite attributes known to scientists. But this interpretation also has its problems. Suppose what some UFO investigators believe to be true becomes very well confirmed: UFOs make no sound, they often stop the motors of cars, they disturb animals that are near them, and so on. Then what is commonly called UFO would be a known physical object and not a UFO. This also seems wrong. To be sure many things may still not be known about them. But there may be many things people do not know about conventional aircraft and these aircraft are not UFOs. Thus, U.S. scientists may not know crucial facts about a new Russian fighter plane. But such crafts are not UFO.

In order to be a UFO what is unknown must be some particular kind of thing about these physical objects. But do people who speak of UFOs have particular kinds of things in mind that is the basis of the lack of identification? I believe that when people speak of UFOs they do have a particular classification scheme or set of categories in mind and their lack of knowledge with respect to this set is their basis for saying that some object is an unidentified flying object. I believe that this set usually consists of the following four categories: (a) material the object is made of; (b) how the object travels; (c) origin of the object; (d) purpose, if any, of the object.

Thus UFO investigators have been unable to determine whether UFOs are made of some sort of known metal, some unknown metal, or some entirely different substance. They have been unable to find how UFOs travel through space, whether they are self propelled, what their source of energy is, and so on. Investigators do not know whether they originate from the Earth, from our Solar System or beyond. UFO investigators are puzzled about the purpose, if any, of UFOs. For example are they manufactured objects with some humanly understandable purpose, e.g. reconnaissance, or are they natural objects with no more purpose than a comet?

If it is unreasonable for a scientist to believe in the light of

present evidence that an object can be classified in terms of categories (a)(b)(c)(d) above, the object would certainly be a UFO. What one should say if an object could be reasonably classified under some categories and not others would depend on the sort of evidence one had and the particular categories at issue. For example, if one only had evidence that the object came from the general vicinity of Jupiter, UFO terminology might still be valuable. If one had evidence that the object came from some artificial satellite orbiting Jupiter one might infer that the object itself was artificially created and served some purposes, perhaps of reconnaissance and research, of some extra-terrestrial being. One might well give up the UFO terminology and speak instead of ETI space probes. However, if one discovered that the object was manufactured of some presently unknown substance and traveled by means of some power source that was beyond our technology, but one did not have any idea about the origin of its purpose, UFO terminology may give way to ETI space craft terminology. If, on the other hand, one only knew the object was made out of some metal, UFO terminology might still be appropriate.

So far I have defined UFO and I have clarified the classification scheme under which UFOs are usually classified. What remains to be defined is a UFO report. A definition of a UFO Report should, I believe, have two conditions built into it. First, it is a report of a person who investigators have good reason to suppose sincerely believes what he or she reports. Secondly, it is a report that investigators have good reason to suppose is really about a UFO. The first condition rules out fraud and hoax perpetrated by the reporter; the second condition rules out fraud and hoax perpetrated by people other than the reporter, psychological delusions and failure to identify things like weather balloons, conventional aircrafts, the planet Venus and so on.

R is a UFO Report relative to reporter P, investigator I, classification scheme S and evidence E if and only if (1) it is reasonable in the light of E for I to suppose that R is about a flying object and what R is about cannot be classified under S; (2) it is reasonable in the light of E for I to suppose that P believes P has seen a flying object that cannot be classified in terms of S.

This definition has the following implications. A report may be a UFO report relative to one body of evidence and not relative to another. Thus a person may be said to have given a UFO report in the light of the evidence that is available now. But new evidence may show that the report is really about a new airforce fighter or based upon some psychological delusion; it would not be a UFO report at all in the light of the new evidence.

NOTES:

1 Jacques and Janine Vallee, Challenge to Science: The UFO Enigma (Chicago: Henry Regnery Co., 1966), p. xv.

2 Edward Condon, Scientific Study of Unidentified Flying Objects (New York: Bantam Books, 1969, p. 9.

3 J. Allen Hynek, The UFO Experience (Chicago: Henry Regnery Co., 1972), pp. 3-4.

4 Ibid.

5 Ibid., p. 10.

LETTERS

MALCOLM DEAN'S RESPONSE TO IVAN W. KELLY & DON H. SAKLOFSKE'S REVIEW OF THE ASTROLOGY GAME:

Of The Astrology Game, David A. Rodger, former director of the Vancouver Planetarium, wrote:

"...while I disagree with some of its conclusions, I must give (Dean) credit for having written the most literate and well-informed book on the subject I've ever read."¹

George Nickas, a Vancouver astronomer, commented:

"...the author provides some ground for a truce in the long battle between astrology and adversary...the presentation of some of this evidence notably recommends the book not only to scientists but also to the reader who would like to go beyond his daily newspaper fortune and get inside the controversy...My own familiarity with Gauquelin's work, with his sound methods and sure conclusions, has long left me puzzled about why his startling correlations have for so long been unknown to or ignored by scientists."²

The Library Journal called The Astrology Game an "intelligent and well-researched work...objective...informative..."³

In contrast to these and other comments, Ivan W. Kelly wrote in The Skeptical Inquirer:

"...there is little here to recommend."⁴

And in The Zetetic Scholar, Don Saklofske has written:

"Dean accuses others of poor scholarship while indulging in it himself..."⁵

What accounts for the strong differences between the opinions expressed regarding my book? Why, clearly, the prejudice which each reviewer has brought to the subject in advance of reading The Astrology Game. I find none of Kelly's or Saklofske's points to have real weight. What is of great interest, however, are the points they have conveniently ignored in print.

The Astrology Game was actually written as two separate works. One, consisting of Chapter 10, 11, and Appendix B, is a presentation of the research of Drs. Michel and Francoise Gauquelin, and an account of their fate at the hands of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP). At the time of writing, this account was one of the few publicly available exposees of the positive results of the Zelen Test, and CSICOP's attempts to suppress news of their failure to disprove the Mars Effect. To these chapters, written with the assistance of both the Gauquelins and our present editor, Marcello Truzzi, were added a complete bibliography of all of the Gauquelins' publications, available nowhere else.

It is the responsibility of a book reviewer not only to fulminate against a work of which he disapproves. He must also give the reader a reasonable impression of what the book contains beyond the points he has chosen to criticize.

Mention of bibliographical addenda, footnoting and similar details is de rigueur in a competent book review.

Is it not curious that Saklofske fails to mention these details, except for one sentence which reduces the work of the Gauquelins to "statistically significant but marginal relationships between personality variables and planetary positions..."? For his part, Kelly calls Chapter 12 "... a sensationalized attack on members of CSICOP," and manages to keep a straight literary face as he glosses over the coverup of the Zelen Test results.

The purpose of such tactics becomes clear when we notice that Saklofske's review acknowledges the "computations and help" of Kelly. The two reviews, in fact, are virtually one hymn from the CSICOP creed. Those who hold this creed have devoted themselves during the past few years to continual denial of their failure to disprove the Mars Effect, to blocking Gauquelin's attempts to have his points and rejoinders published promptly and completely in The Skeptical Inquirer, and to maintaining an effective coverup in the press, which is only now weakening.⁶

Reviews of paranormal books published in The Skeptical Inquirer (and now, unfortunately, in Zetetic Scholar) manage to have a surprising, and surprisingly deadening, tone. The subliminal message is always, "don't bother with this one, there's nothing there, aren't you glad you read this review rather than bother with such garbage?" Such reviews are, in fact, sermons to the converted, and serve no real usefulness in increasing understanding between parties to the various paranormal issues.

The extent to which this mentality unwittingly overtakes itself in re-viewing such a work as The Astrology Game is vividly illustrated by Saklofske's exasperated comment: "Throughout the book, it is unclear to which astrology Dean attaches himself."

The answer is neither, and practically all of Saklofske's and Kelly's objections hinge on this one point. I am continually accused of logical fallacies and "questionable devices" when I was actually exposing the reader to many types of arguments which are raised on all sides of the astrological debate. On page 335, I made this quite clear by stating: "For me, as a journalist, the real fun comes from covering this field as it develops...My role is that of an astrology critic - like a movie critic - who attempts to point out and assess current developments."

That was the aim of The Astrology Game, and as such an introduction, the book succeeds. Readers should not approach it looking for the bible-thumping nay-saying we have come to expect from fellow-travellers of the CSICOP. I continually invite the reader to come to his own conclusions about the various issues, and an important theme underlying the entire book is the role of consensus realities and paradigms in determining in advance the conclusions one is likely to reach.

The only outstanding issue which remains is Saklofske's defence of Ianna and Culver's data on Jupiter-Saturn conjunctions. Since these authors argued against a cycle in Presidential assassinations, it is rather strange that they would not use the original astrological frame of reference in their analysis. In making a criticism of sun-signs, for example, it would be rather foolish to use the equator rather than the ecliptic in statistical

work. Yet that is precisely what Ianna and Culver did. Their dates of conjunctions are indeed taken from an astronomical reference, not an astrological one, and they do not even bother to inform readers of this fact.⁷ Saklofske then glosses over the remaining inaccuracies in Ianna and Culver's table.

It must have been frustrating indeed for these critics to see my main point about the circularity of arguments pro and con astrology underlined by the attempted assassination of Reagan! Astrologers had been holding their breaths, wondering if the cycle would hold if the conjunction occurred in an Air sign. The universe's response was a beautifully ambiguous and as designed-to-frustrate-skeptics as is astrology itself: Reagan was shot, but (so far) he has survived!!

As I concluded in The Astrology Game, "The indications are that a new astrology is already being born, and that the public may now be ready to hear about it. Two groups, especially, will resist these developments - the traditional astrologers and the establishment skeptics. Neither will examine the evidence from a creative point of view, seeking a new synthesis, because this would imply the death and transfiguration of their old world-view. To both groups, my heartfelt condolences."⁸

FOOTNOTES

¹ Vancouver Province, 4 January 1981, p. 7.

² Vancouver Sun, 6 February 1981, p. 137.

³ Library Journal, 1 February 1981, p. 359.

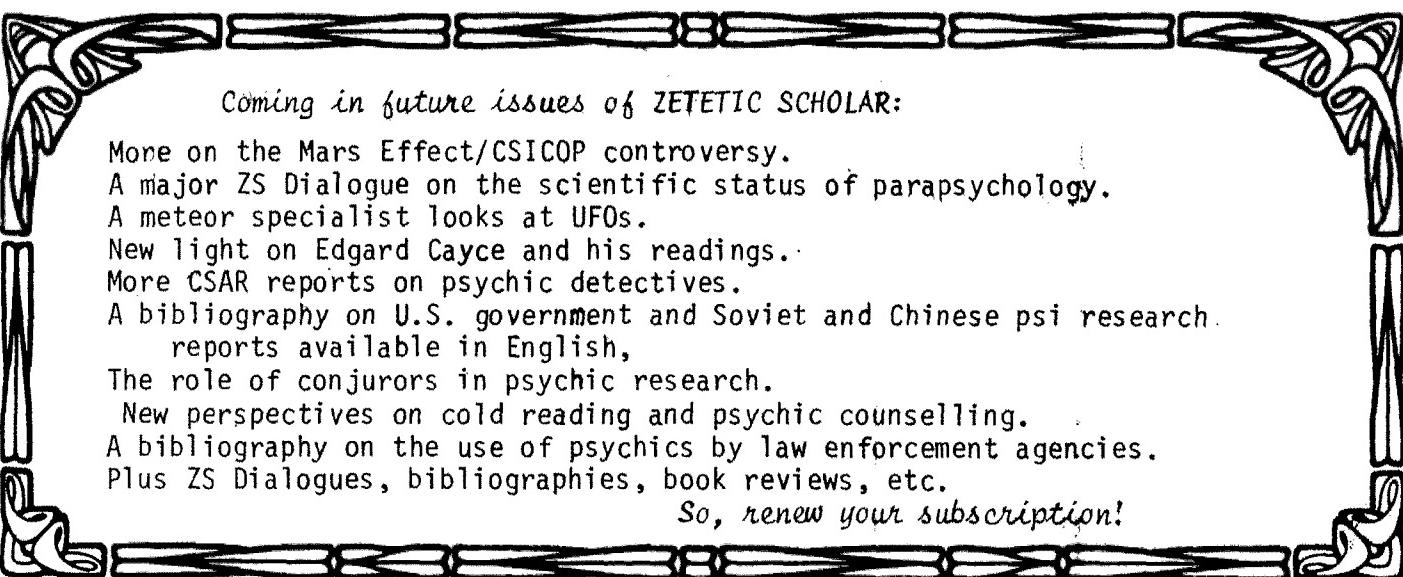
⁴ The Skeptical Inquirer, Summer 1981, pp. 60-65.

⁵ Zetetic Scholar #8, July 1981.

⁶ See the somewhat inaccurate and biased report in New Scientist, 29 October 1981, p. 294.

⁷ Culver, R. B. and Ianna, P. A. The Gemini Syndrome: Star Wars of the Oldest Kind, Pachart Publishing, Tucson, 1979.

⁸ The Astrology Game is available from Beaufort Books, 9 East 40th St., NY, NY 10016. (212) 685-8588; or, in Canada, from General Publishing, 30 Lesmill Rd., Don Mills ON, Canada M3B 2T6.



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IVAN W. KELLY REPLIES TO MALCOLM DEAN:

Malcolm Dean's reply to the Skeptical Inquirer¹ and Zetetic Scholar reviews of his book, The Astrology Game³, leaves much to be desired. His rebuttal fails to address the main criticisms of his book. Instead, it attempts to divert the reader from these main issues with other reviews, with irrelevant arguments and, (predictably) with a series of defensively stated informal logical fallacies.

Dean's simplistic explanation for conflicting reviews (reviewer prejudices) does nothing to dispel our four substantive criticisms which were based on scientific research and objective information. He failed to defend the following criticisms: (1) The Astrology Game contains much misleading information. These were amply documented in the two reviews and understandably ignored in Dean's response. For example, Dean chose not to defend the statement: "Psychologists are slowly becoming aware of a growing number of studies which have obtained positive results for astrological hypotheses."⁴ Of twenty articles on astrology that have been published in prominent psychological journals since 1977, only five reported positive results; and there is strong evidence that three of these, which deal with an extraversion-introversion zodiac relationship, are due to non-astrological factors.⁵ (2) The Astrology Game demonstrates a limited understanding of elementary scientific method. A number of recent astrological techniques for predicting future trends (Astro*carto*graphy, Barbaultis research) are described by Dean in The Astrology Game as "promising."⁶ In reality, they are based on all sorts of methodological flaws. No attempt was made by Dean in his response to invalidate the criticisms. (3) The Astrology Game is structured around fallacious reasoning. Examples of these (faulty analogies, begging the question, appeals to pity, etc.) were documented in the reviews. Dean's contention, that he was "...actually exposing the reader to many types of arguments which are raised on all sides of the astrological debate"⁷ is a pathetic attempt to justify his reasoning. If Dean really was attempting to present types of arguments, he failed by neglecting to draw the readers' attention to the fallacious aspects of those arguments. No sources were given for the majority of fallacies in the body of the text so that the reader is led to assume that they express Dean's opinions.

Dean's prejudice against CSICOP leads him to commit the genetic fallacy and to dismiss the negative reviews of his book as "...virtually one hymn from the CSICOP creed."⁸ This is typical of the assumptions which pervade Dean's writings. (4) In The Astrology Game critics of astrology are denigrated. Dean's use of epithets ("small minds," "True believer (disbeliever)," "dishonest," "rabid skeptics," "fanatical debunkers") scarcely adds credibility to the authenticity of his "(invitation to) the reader to come to his own conclusions about the various issues...."⁹

Those already acquainted with the field will not find in the book anything new, others will not even find a sober and sensible introduction to the controversy over astrology. Rather, they will find a book in which criticism of opposing views is never courteous, much negative evidence against astrology is conveniently left out, studies statistically and methodologically flawed are presented to the reader as valid and supporting astrology, and anecdotal "evidence" from a mixed variety of sources is presented as if it was strong evidence.

The Zetetic Scholar review stated, "Throughout the book, it is unclear to which astrology Dean attaches himself."¹⁰ Dean's reply is:

"The answer is neither, (sic) and practically all of (the) objections hinge on this one point."¹¹ But how does this fit in with what he says in The Astrology Game: "I'm convinced that planetary influences do, indeed, exist"¹² and "An astrological revolution is well underway,..."¹³ Dean's entire case is built on the marginal, and partially investigated claims of the Gauquelins. Almost all of the studies and replications of the Gauquelins' claims have been conducted by the Gauquelins themselves. This is not a very satisfactory state of affairs. Hopefully, it will change in the near future when other scientists investigate the claims on independent populations to those considered by the Gauquelins. It is also not clear why the findings of the Gauquelins, if valid, should be called "astrology." The Gauquelins describe their findings without the astrological symbolism and as Eysenck has pointed out:

I think we must admit that there is something here that requires explanation. Whether that explanation would be along astrological lines is, of course, another question--indeed, astrology does not furnish us with an explanation at all, it simply asserts¹⁴ the facts (or something very much like the facts) actually found.

This issue is further considered in my Skeptical Inquirer review of Dean's book.

In his response to the reviews, Dean informs us that "...an important theme underlying the entire book is the role of consensus realities and paradigms in determining in advance the conclusions one is likely to reach."¹⁵ The term "paradigm" is defined in terms of the equally unclear; it appears to mean something like "a consensus view of reality,"¹⁶ or a "presently accepted model of reality."¹⁷ This underlying theme is nowhere clearly articulated. (Is it true that what reality is is determined by consensus?) All we are offered are uninformative appeals to authority from physicists Fritjof Capra and Bernard d'Espagnat, and rather uninformative statements. For example, after giving some anecdotes from individuals who have "seen astrology work" he tells us, "But the usual attitude to such examples, depending on your paradigm, is 'Wow! It really works' or 'Such nonsense'"¹⁸ After discussing Gauquelin's findings on sports champions and the Mars effect he says:

A traditional astrologer would assert that those successful champions who do not have Mars in the key sectors would have other combinations of planets, signs, and aspects to provide them with an assertive, aggressive disposition. Gauquelin would simply put these cases down to fluctuations within a statistically significant tendency to have Mars in key sectors.
It all depends upon your paradigm.¹⁹ (Italics mine)

Dean appears to be stating that truth is relative to one's paradigm or what those of a particular persuasion believe by consensus. We will call the statement underlying Dean's paradigm view the Belief Principle (B.S. pr.). Dean's conceptual relativism (?) entails this principle and this principle is untenable.

Proof:

- (1) If the B.S. pr. is true, then the belief "'The B.S. pr. is true' is true" is true.

- (2) If the B.S. pr. is true, then the belief "'The B.S. pr. is true' is false" is true.
- (3) Hence, since the B.S. pr. implies that it itself is both true and false, it is self-contradictory.

Those of a relativistic persuasion should resist the temptation to dismiss the foregoing refutation on the ground that it assumes truth is absolute when it is in fact a matter of opinion (i.e. relative). For such an argument only serves to make even clearer the inherent inconsistency in the relativist's position. Such a defense of the B.S. pr. is self-defeating; it is an ill-fated attempt to defend on contradictory claim by another.²⁰

The present journalistic trend toward investigative reporting can lead journalists into areas in which they are ill-equipped to interpret the situations objectively. The scientist has a responsibility to bring to public awareness the flaws in their interpretations.²¹

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- ³M. Dean. The Astrology Game. New York: Beaufort Books, 1981.
- ⁴Ibid., p. 205.
- ⁵I. Kelly & D. Saklofske. "Alternative explanations in science: The extraversion-introversion astrological effect." The Skeptical Inquirer, 5(4), 1981, 33-37.
- ⁶M. Dean. The Astrology Game, Ch. 12.
- ⁷M. Dean. "Response to Kelly and Saklofske," Zetetic Scholar, 9, 1982.
- ⁸Ibid.
- ⁹Ibid.
- ¹⁰D. Saklofske. "Review of The Astrology Game," Zetetic Scholar, 8, 1981, 133.
- ¹¹M. Dean. "Response...,"
- ¹²M. Dean. The Astrology Game, p. 99.
- ¹³Ibid., p. 131.
- ¹⁴Quote found in M. Dean. The Astrology Game, p. 227.
- ¹⁵M. Dean. "Response...,"
- ¹⁶M. Dean. The Astrology Game, pp. 52, 56.
- ¹⁷Ibid., p. 46.
- ¹⁸Ibid., p. 56.
- ¹⁹Ibid., p. 246.
- ²⁰I would like to thank Professor Rudi Krutzen for this argument against conceptual relativism.
- ²¹Dean's comments on Culver and Ianna's data on "presidential cycles" is not covered here. I have invited Culver to respond to Dean. However, I would suggest the reader re-read the relevant section in the Zetetic Scholar review (e, p. 135) and then look at Dean's comment in his response.

ZS Dialogues Continued

RICHARD DE MILLE COMMENTS ON J. RICHARD GREENWELL'S REPLY (ZS #8) TO GEORGE O. ABELL, RE "THEORIES...OF UFOS":

In his response to Abell (ZS #8), J. Richard Greenwell chides Carl Sagan for inconsistency, in both admitting the astronomical improbability of multiple human evolution and confidently expecting to find intelligence throughout the universe. Sagan, however, is on record as a staunch supporter of neo-Darwinian natural selection and of intelligence as eminently selectable. It is not human beings he imagines on distant planets, simply intelligent beings. Though I think Sagan has misplaced his confidence, in a moribund evolutionary theory, I see no inconsistency in his position.

Greenwell advises some astronomers that attribution of directed purposefulness to organic evolution is a religious act, which they "should not attempt to cloak ... in scientific respectability" -- but is their vague directed purposefulness any less respectable than a vacuous, circular natural selection or an undemonstrable bio-field or mysterious jumping genes? Why not face zetetic facts? We have today no viable explanation of evolution, which is the biggest anomaly known to science.

GEORGE O. ABELL RESPONDS TO J. RICHARD GREENWELL'S REPLY (ZS #8) TO ABELL RE "THEORIES...OF UFOS":

In my response to Mr. Greenwell's article I did not mean to imply that there are necessarily many other civilizations in the Galaxy, and on re-reading what I wrote I find that I did not say so. I was speaking to the argument advanced by many adherents of the extraterrestrial hypothesis for UFOs, namely that because there are so many possibilities for life in the Galaxy, it is reasonable to believe that UFOs are interstellar space vehicles. My point was that even if there were, say, a million other civilizations (and I share Mr. Greenwell's skepticism about this), and even if they all had mastered interstellar travel and were motivated to rove about the Galaxy (which I consider to be enormously unlikely), even then we would not expect to have been visited, or at least not often.

The rest of my tongue-in-cheek ideas were meant to poke fun at what I thought were really very foolish hypotheses for UFOs that Mr. Greenwell listed. Of course he doesn't believe any of them any more than I do, but he seemed to list them as serious hypotheses.

On re-reading Mr. Greenwell's article, I see that he did not actually profess belief in even the ETH hypothesis, and in fact what he said about its acceptance by scientists is in substantial agreement with my response to him (save for the remark that scientists should "know better" than to doubt the idea, and that they have an emotional commitment not to believe it).

Evidently each of us is guilty of not reading the others remarks carefully enough, or perhaps of reading into them statements which were, in fact, not made or intended. I apologize if I have misjudged or offended Mr. Greenwell.

REPLY BY J. ALLEN HYNEK TO J. RICHARD GREENWELL'S RESPONSE (ZS#8) TO
HYNEK'S COMMENTS ON GREENWELL'S UFO PAPER (ZS#7):

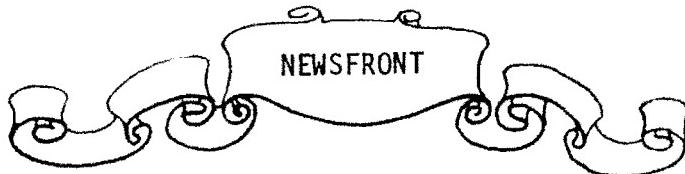
I respect Greenwell's reluctance to assign probabilities at this stage of our investigation of the UFO phenomenon, but as for myself, so long as I do not assign the values of 1.0 or 0.0 for the probability of the accuracy of the reported UFO events, I feel that I am well within the bounds of scientific procedure.

I base my judgement on the comforting thought that the same "human perceptual system (that) is very much subject to socio-cultural influences" is operative in all areas of life, such as when a witness gives visual evidence in court or when a person describes an adventure encountered in his travels. Yet we do not reject the aggregate of evidence presented in court or discount all travel adventure stories because some may be the result of a faulty perceptual system.

Thus I do not find any compelling reason to reject all of the thousands of UFO accounts which come to us from all parts of the world from people who are judged responsible and sane by commonly accepted standards.

Further, this "perceptual system" does not apply --at least not in the same way-- to radar returns, to photographs and to actually observed and recorded physical effects (like skin burns, falling hair, conjunctivitis, etc.) reported as a direct, observed consequence of a UFO encounter (Close Encounters of the Second Kind).

I concur that my assessment of the probabilities may change after we have a better understanding of the world and universe around us, It has always been so.



- * The new Society for Scientific Exploration, formed for the study of anomalous phenomena by Prof. Peter A. Sturrock, will be holding its first general meeting on June 3-5, 1982, at the University of Maryland. Its new journal is scheduled for publication early in 1983. Its editor, Ronald A. Howard (Prof. of Engineering-Economic Systems at Stanford University) is soliciting articles.
- * The new International Society for Cryptozoology recently had its formative meeting and is now inviting applications for membership. Interested parties should write to J. Richard Greenwell (Sect. and Treas.,) at P.O. Box 43070, Tucson, AZ 85733. The ISC President is Dr. Bernard Heuvelmans and its Vice-President is Dr. Roy Mackal.
- * A new journal featuring reports on parapsychology in the USSR and China has issued its first number. Psi-Research is edited by Larissa Vilenskaya. For information, write to: 3101 Washington Street, San Francisco, CA 94115.
- * The newly formed Association for the Scientific Study of Anomalous Phenomena (ASSAP) publishes a journal, Common Ground, already in its fourth number, which should be of special interest to ZS readers. For information, write: Kevin and Sue McClure; 14 , Northfold Road; Knighton, Leicester, U.K..

REFLECTIONS ON THE ROLE OF HYPEROSMIA IN ESP: SOME PERSONAL OBSERVATIONS

MICHAEL HARRISON

In the traditional restriction of the physical senses to five - touch, sight, hearing, taste and smell - it has long been recognized that the last two, taste and smell, are so closely connected that this pair might well be considered as but one sense. It is true that, deprived of the sense of smell, a person may still distinguish the differences of taste among those substances which are sweet or sour; salt or acidulous; so that, even if the senses of smell and taste be obviously so closely related as to be, as it were, interdependent, it is also obvious that smell must be regarded as having a claim to be considered a sense in its own right. What does not seem to have struck the investigator of human sensation is that these five senses are, in fact, all variants of the sense of touch - and, of them all, the sense of smell has been studied least of all.

"HALLUCINATION" AND ESP

That there are hallucinations, even those who have never endured the disturbing experience of "seeing what we know to be absent" will admit; but all too often, the significant vision - the "vision with a message" - will be dismissed, even by the patient, as mere "Hallucination." The fearful delirium of malaria and other delirium-inducing fever - an agony that I have known only too well - has done much to provide the doubters of ESP with effective "rational" ammunition, enabling these doubters to attribute every sense-experience of which the stimulus is not immediately apparent or explicable to some "hallucinatory" product of bodily imbalance.

I am well aware, as so many of my private correspondents complain, and of which so many writers on the Paranormal complain, too, that it is almost impossible to discuss, in a generalized manner, experiences which are inevitably restricted to a small body of "sensitives." I have had, in a long life, only six paranormal experiences shared with another person; that is, having a person near me, and sharing my experience at the same moment. Four of these involved the voice calling a name - an experience that Dr. Johnson mentioned to Boswell, adding that he had investigated the phenomenon most thoroughly, but had found no significance in it - as, indeed, has been the case with me. The two other cases of shared paranormal experience involved, first, a tremendous explosion waking both my wife and me from a deep sleep in a London dawn - an explosion so violent and so noisy that it shook the house and rattled the windows and left us with our ears ringing. But when I opened a window and leaned out, there was...nothing: no anxious neighbours at window or front-door; no clang of the racing fireengines. Nothing. And there never was anything. No-one but we two had heard the echoing thunder of that explosion. What did it mean? Was it merely, as I have been told, a "telepathically shared" hallucination (but even that's a bit unusual,

*Viewpoints essays are published primarily for their speculative interest and are not intended as rigorous scientific documents. -- MT

surely?), and that, because we could not attribute any message to the experience, that experience was then without significance.

SMELLING MORE THAN RATS

The last of my six shared experiences was that of our seeing together what appeared to be the apparition of a small, female-shaped brown cloud, as it came through the front-door of our flat in Victoria, London. We had both seen this shape several times earlier, but never had we seen it together. For some reason unknown to me, my wife did not care to discuss this unusual experience, and when I asked her to confirm it to Colin Wilson, she did so, but with evident reluctance. I mention these shared experiences because I am about to discuss yet one more phenomenon from my personal spectrum of ESP, and to state that, so far, I have experienced it with and without detectible "significance," and that I have yet to share the experience with another human being. I refer to my life-long hyperosmia.

Hyperosmia, expressed at its crudest, is what many would define as "smelling that which isn't there"; and, when no detectible significance may be associated with its manifestation, it may, by the unbelievers, be comfortably dismissed as hallucination. And here again, we meet with the prime difficulty in all exminations of the paranormal: the difficulty - often the impossibility - of describing purely personal, purely subjective, experiences in such a manner as to interest and, if possible, to convince others who have not had, and never may have, such experiences. But I do experience hyperosmia, and here I may treat of the phenomenon in admitting that I have no hope of convincing the unbeliever, but may well hope to interest those who, whilst yet to be convinced of the existence of hyperosmia, do not need to be convinced that there is a wide range of phenomena to be encountered in even the most superficial acquaintance with the Paranormal.

A SCENT OF LILAC

Before I go on to discuss the more generalized aspects of my hyperosmia, I would like to give, in some detail, my first acknowledgment of a significance in my hyperosmia. The experience happened in the dawn of an autumn day in October, 1930, and there may be a fact of importance, that I shall discuss shortly, in the map coordinates of the mediaeval house in which I was then living.

I was a young man, and going through all a normal young man's pre-occupation with the more enchanting members of the opposite sex. The contemporary object of my hardly stable fancy at that time was a lady who lived some four or five miles out of the ancient Roman city in which I lodged. And, I knew, I should not see her for some days, since she had gone up to London.

At exactly three-fifteen in the dawn, I was awakened by a terrible sensation: I was choking. I fumbled for the overhead switch, turned on the light, and, leaping out of bed, made for the ancient casment window, the two leaves of which I flung open, leaning out and thankfully gulping in the chill early-morning air.

Behind me, and rolling over and at the side of me, was a chokingly thick cloud - invisible but only too palpable - of scent. It was as though someone had smashed, not one, but many of those giant bottles of scent that the more expensive perfumers display in their windows - always too big and too dear for any but the very rich man's pocket. (I was obviously not the first to have had this experience. Poe, the para-normally disturbed, has left us a record of his own hyperosmia:

Then suddenly the air grew denser,
perfumed by an unseen censer,
Swung by seraphim whose footfalls
tinkled on the tufted floor...

Apart from my hyperosmia, I have always had an acutely developed sense of smell, and have no difficulty in distinguishing among the scents, perfumes, bouquets - call them what you will - of wine, tobacco, scent, and so forth. So, as I leant out of the window in that chill dawn, I recognized at once what scent it was which was quite intolerable in its strength. It was the famous Lilac, made by Floris, of Jermyn-street, St. James's - and I knew of only one of my female friends who loved this scent to the exclusion of all others. At nine o'clock on that same morning, I telephoned her.

"Good heavens! How did you know that I was back?"

"If I told you, you wouldn't believe me. But you got back at exactly three fifteen this morning... Didn't you?"

"I don't know about the 'exactly'; but it was about then, yes. But how did you know that I suddenly got fed up with London, and on the spur of the moment decided to jump in the car and come back...?"

I told her later in the day. She didn't "exactly" accept the fact of the "significant" hyperosmia, but, on the other hand, it was a bit odd, wasn't it... ?

Now all this took place in and near the ancient Roman city of Colchester - Camulodunum. And this city lies on the most active of Britain's seismic faults: I was once caught in a mild earthquake there (a most disturbing experience), and in 1884, a quake shook the spire of the Methodist church tumbling to the ground.

In a recent book of mine on unexplained disappearances, usually of persons, I point out the apparently close relation of these disappearances to the proximity of seismic faults. Was this particular "message-delivering" hyperosmia - which came very close in time (a matter of days) to my first experience of an earthquake - related to the presence, and, at that time, the very active presence, of a seismic fault?

ODOURS, PLEASANT AND OTHERWISE

In my case, the range of odours assignable to hyperosmic classification is far narrower than one might expect. I have smelt some appalling odours: odours with the deathly menace of Hell implicit in their detestable horror; but frightening, even merely unpleasant, odours are rare in my experience. I mostly smell what is pleasant - scents (always the most expensive: I

wonder why?) of every kind; and incense though never, save once, in the vicinity of a church (which did not use it). The last is by far the commonest of all the odours which come my hyperosmic way.

But there is one unpleasant smell whose "extended" significance I did not realize, I admit, until I had begun this article, for all that the smell was first detected by me several years ago. I refer to the smell which seemed to come from both the hot and cold taps in the kitchen here: from both came water which smelled of those odorous waddling old black dogs which wander up and down the sands, shaking the sea-water off their evil-smelling pelts as they come across anyone trying to take a sleep or a suntan by the sea. I wrote a letter to the local newspaper some five or six years ago, asking the Editor to ask the local Water Board to explain the smell of "old wet black dog" in the drinking-water laid on at my flat and (as I have found) elsewhere in Hove. But I made the mistake of using an ironic style; and so the letter was not printed. But I remember that phrase, "old wet black dog," and only a short while ago, as I said, the significance of the phrase suddenly struck me. For, as I see by turning up pages 132 and 133 in my friend, Colin Wilson's fascinating Mysteries (New York edition) that it isn't only into tap-water that the paranormal manifestations of black dogs insinuate their malignant selves. The passages from Wilson's book are too long to be quoted in full here, but he mentions:

...a photograph of a church path at Bishop Cannings [a village] in Wiltshire (not far from Stonehenge) where a black dog is often seen to run across the road. The apparition of a black dog is associated with the Rollright stone circle in Oxfordshire - a site linked persistently with witchcraft, even in modern times; the dog was actually seen by detectives investigating the savage 'witchcraft murder' of Charles Walton in a nearby field in 1945. Lethbridge devotes several paragraphs in Witches and subsequent books to apparitions of dogs...he would have been fascinated by the researches of Ivan Bunn, a collector of black dog legends in the East Anglia area. Bunn noted that almost all apparitions of black dogs - and he collected over forty from the same fairly small area - were seen near water,* either the sea or rivers, and on low-lying (i.e. damp) ground. 'In about fifty per cent of these accounts, the witnesses state that shortly after their encounter with the black dog a close relative has died suddenly.'

.....
But why black dogs...? Black dogs were associated with Diana, the witch goddess, whose cult was particularly strong in country areas. It is easy to see why their image should be associated with such areas. But why as harbingers of death? Because some level of the mind already knows⁺ about the future; this is its method of conveying the information symbolically...

* My italics - M.H.

+ Mr. Wilson's italics

Now there are several points in this important pair of quotations which are of precise relevance to my subject, though one must admit that, as an instrument for "conveying...information symbolically," hyperosmia is far from precise. But let us take the facts of the canine effluvium in the tap-water...

Facts...? Well, yes. As follows...

1. To identify a smell as being of a canine type would not be difficult; in fact, so far as anyone who had ever owned a dog was concerned, this identification would be inevitable. But, in my case, I instantly identified the offensive smell as that of a sea-water-logged black dog, and called, in a letter to the Editor of the (Brighton) Evening Argus, the infection of the mains-water by the smell of a black dog. The smell "followed me" outside the flat. I sent back a glass in the local hotel-bar because of this smell, but the replacement still smelled of dog, and the fact is that no-one to whose attention I called the stench detected it. The smell, and its message, were for me alone.
2. Traditionally, the black dog "apparitions" manifest themselves near water. In my case, the dog could hardly have been nearer: he was actually - in essence, at least - within the water; but our flat lies only a few hundred yards from the sea, which is visible through the south-facing windows. However, in my studies both of Spontaneous Human Combustion and Unexplained Vanishings, I note the consistent proximity of the victim to either water (sea, river, lake) or, in fewer cases, seismic fault.
3. "...a close relative has died suddenly." The dog-smell in the tap-water began to manifest itself immediately after (though I have had to wait until now to make the connection) my wife had been told that her illness was terminal. Perhaps the imprecise nature of the communication may be explained by the fact that she did not die at once, but survived for two more years. My own "personal" ESP includes much more precise information-conveyors: for instance, the falling from the walls of (usually) a picture or some other ornament leaves me in no doubt that someone close to me has died, though I have always to wait to learn the identity of the deceased.* (This happened to me exactly a week-ago.) Here again, we observe the imprecision of the majority of ESP communications: we know that some information is being communicated...but what? Only now, six years after I first smelled the black dog do I find the nature of the information being - or attempted to be - communicated; and that because I recalled that Colin Wilson had mentioned black dogs.
4. Wilson's reference to the connection between black dogs and Diana, the witch-goddess, is curiously significant, for me. Besides my worry over the reports that my wife was bringing back from the various medical specialists that I was coming more and more to

*In the case of my wife's actual passing, this was announced, on the previous night, by a violently loud screeching of gulls over the house, precisely at midnight.

mistrust, I had begun to collect the necessary material for a completely original history of King Arthur. And what has he to do with black dogs...? Well, the Greek name of (Roman) Diana is Artemis, and my research seemed to confirm that the Roman family of the Artorii, of which Arthur (Artorius) was a member, were hereditary high-priests of Artemis, the virgin Bear-Goddess. I choosing Arthur as a subject for my pen, I was, as my wife pointed out, risking a great deal of what would almost certainly be unrewarding work. "You'll never get any publisher to accept the fact that your book will be original...and as for unoriginal books on Arthur, there are far too many, at any rate." And, so far, she has been right; my Arthur has had no takers. I mention this book because the worry attached even to its planning merged, in time and place, with the worry over my wife's grave illness. And now I ask myself: did the unheeded (because not understood) "message of the black dog" concerning my wife also carry with it a message about my work on Arthur? Was I being warned off an unsaleable project...?

INTERPRETING HYPEROSMIC INFORMATION

In the past few weeks, and especially in the past week, the small, apparently insignificant manifestations of "precognition" have been as impressive as numerous - though they still remain without apparent significance.

For instance, what is the significance of these three - selected at random - precognitions?

1. A word, stenodyne, that I have never heard before, echoes in my mind. I instantly contrive an "introductory" situation, by which (I was for many years in advertising, and was always being called upon to invent new names) I had been called upon to invent a trade-name for a firm offering their customers new and improved standards in speedy service. Hence somebody else's proposed "Stenodyne," explained as "speedy effort...speedy activity." I was clearing up an immense amount of old papers at the time; and I said to myself, "No: 'Stenodyne' won't do. 'Stenos' doesn't mean 'speedy', no matter what 'stenographer' has been accepted as meaning. 'Stenos' means 'narrow, strait' - the word needed here is 'tachys' - 'quick, fast, fleet, speedy'. The trade-name we need is, not 'Stenodyne' but 'Tachodyne'. "I picked up a folded letter - an old one. I had no idea what it was. It was a bill from a London restaurant. The proprietors' name, written boldly across the top of the bill was...Tachwood Limited.
2. An American reader kinkly cuts out and sends to me items of interest from his local (Asbury Heights) newspapers. I was sorting a mass of letters, etc., into various manila folders; one in front of me was marked with the name of my Senior Trustee, Mr. Tisdall. I picked up several press-cuttings that I had not yet looked at. I opened the top cutting: TEXAS GULF COAST FEARS THALLIUM "TIME BOMB" - and the story quotes "University of Texas Poison Control Center investigator Ron Tisdell..." - only different by one vowel from the name of my Trustee.

3. Thinking (why...?) of Evelyn Waugh, I tried to remember the name of a novel of his that I had not read. What was it? - "Officers and Gentlemen"...? Something like that. And the name of the hero...? Guy Cruikshanks...? There was a King of that name: Richard Crookshank. I'd never thought of it before, but the nickname must mean that he had bandy legs - probably through untreated rickets as a child.

I was wrong...twice. That wasn't the name of Waugh's hero. And we did not have a King nicknamed "Crookshank" - a "Longshanks," yes, and a "Crookback" - but never a "Crookshank", a bandy-legged rachitic. But in what category of "mistake" must we put this error? For it "matched itself up", as Jung might have said, when, later in that day, I went to lunch with some friends, and in their house, whilst the lady was preparing the food and her husband was telephoning, I opened his copy of Popular Archaeology, and read there that, when they recently discovered the tomb of Philip II of Macedon, the iron greaves found in the tomb were not symmetrical, indicating that the King suffered from some malformation of one leg...Philip Crookshank.

Now all this is interesting, but, to be blunt, what does it all mean? That we are being led, through the observation of one fact, to the encounter with another, related fact, hardly gives either any significance. (Or so it seems to us.)

As I said, the odours that my hyperosmia brings to my nostrils are, in the main, pleasant; I am always smelling the most delightful scents, and I can mostly identify each. But - and here's the rub! - I do not contrive to (perhaps am unable to), not only relate these scents to their known-to-me wearers, but even where I am able to do this (I frequently smell my wife's favourite scents), I can extract no "message" from the recognition.

To some extent, this is true of all prediction - as they found who went to consult the Delphic Oracle or the Sibylline Books: that its very vagueness, no matter how the message be communicated - almost always makes the message incomprehensible to the point of uselessness. One must first learn the code.

And how is this to be done? Well, I have learnt that a falling picture or other object descending from the wall betokens a death; and now, through Colin Wilson, I have learnt of the connection between the black dog and imminent death.

Should the dog return, I shall look for the fact that he is trying to communicate. But what of those other odours which make up the repertoire of my hyperosmic experiences? How may I have enough experience - live long enough - to interpret all? And, in that case, what is the purpose of hyperosmia and all the other "symbolic" methods of conveying information? It is like shouting a warning to another in a language that he does not understand...

WHEN "SUBJECTIVE" BECOMES "OBJECTIVE"

I have referred to what I may justly call my "personal hyperosmia," but there are many well-attested occurrences of a phenomena that we might term "group odour-detection" sometimes of an unpleasant character, sometimes of quite the reverse; and if the generally-accepted view is that hyperosmia is a purely subjective "hallucination," then behind that detection by a large number of people, in the same place and at the same time, of some distinctive odour, must lie, one should argue, an odour-source completely objective. Are we then to distinguish between two types of phenomena, each affecting the olfactory nerves, but in one type of phenomenon, observable only individually (the so-called hyperosmic hallucination); the other, the perception of the smell by more than one person? This type of "group perception" is generally --though not exclusively--associated with Poltergeist activity, especially of the more violent kind. Hundreds of such case-histories are available, so that the Poltergeist's existence--though not (yet) its nature--is considered by me to be "proven"; and in my friend, Colin Wilson's latest book, POLTERGEIST! A Study in Destructive Haunting (London: New English Library, 1981), there are several accounts of this "group odour-detection," all associated with Poltergeist activity, from which I have selected five of the most interesting. For the fascinating details of the Poltergeist activity associated with the foetor or fragrance detected by a large number of people who witnessed the violent activities of the Poltergeists, I refer the reader to Mr. Wilson's books; here I am concerned with hyperosmia, and not with Poltergeists - though that one phenomenon may hardly be considered independently of the other becomes more and more certain as we examine both.

VIOLETS...AND "THE STINK OF CABBAGES"

The first of my five selected cases took place, appropriately enough, in Robertson County, Tennessee, in 1817; and involved a farmer named John Bell, his wife, Lucy, and their nine children. It is now accepted modern practice to seek for the focus of poltergeist activity in the presence of a (usually) disturbed adolescent, most often a girl. In this case, there seems to be little doubt that Elizabeth - "Betsy" - Bell, aged twelve, provided the motor impulses to set the poltergeist activity in action. Though having taken place in one of the most rural of all American rural parts, the case has been fully documented, and contains items from the almost complete repertoire of poltergeist "tricks," including multiple personalities, voicing threats; "invisible animals" (dog, bird, "rats gnawing inside the walls**"), assaults on the Bell family.

But, though Mr. Wilson does not point this out, this case is unique in that "the Witch" - noisiest and most malignant of all the unseen visitors - and not the harassed Bells, was the entity which detected and complained of an unpleasant odour - that of the Negro slave-girl, Anky. This is the only case that I have encountered in which it is the poltergeist who (which?) detects the odour, and not the victims of its activities.

*Had the late H.P. Lovecraft this or a similar case in mind when he wrote that masterpiece of horror-fiction, The Rats in the Walls?

This case is not, alas, unique in that the principal object of the malign activities, John Bell, died on 19th December, 1820, literally driven to death after three years of "occult" persecution. There have been other cases; with suicides among those deaths...

The next case reverses the reversed: here it was Mrs. Fielding and the many witnesses, including the eminent "psychic-investigator," Dr. Nandor Fodor, and two hard-boiled reporters from the London Sunday Pictorial, who smelt two contrasting sets of odours: "a spray of violet perfume" - accompanied by a fall of fresh violets - and the "unpleasant "zoo odour" as Mrs. Fielding claimed that she was being clawed by an invisible tiger. Mr. Wilson comments: "Mrs. Fielding was, in fact, the 'focus' of the most interesting and complex case that (Nandor Fodor) ever investigated." It was certainly one of the most unusual, with "apports" arriving in the Fielding house at Thornton Heath, a south-eastern suburb of London, as diverse as Roman lamps and pottery labelled "Carthage," white mice, a bird, a silver match-box and (dropped with a crash in the hall) an elephant's tooth.

"THE BLACK MONK OF PONTEFRACT"

Pontefract is a quiet, slowly-decaying town in Yorkshire. It is very ancient; and whether justified or not, the accepted explanation that the name (pronounced "Pumfret") is derived from the Latin for "Broken Bridge" sufficiently indicates the town's claim to a notable antiquity. This was very much Roman Britain; but it was to an entity living a thousand years after the Eagles had departed that the singular events, beginning in August, 1966, were attributed: a Cluniac monk of Pontefract, hanged for rape in the reign of Henry VIII.

That no such clerical criminal has been traced did not affect the attribution: it was a catching newspaper tag, and the case has remained that of "The Black Monk of Pontefract" ever since - and will, one imagines, so remain.

James Branch Cabell once pointed out the very small number of plots that a writer may use; yet this small number has provided us with all the diverse fiction of the world. So, in the activity of the poltergeist, the repertoire, wide but not unlimited in scope, is carefully selected to provide a diversification to make each poltergeist haunting show some specially distinctive quality. In the case of "The Black Monk" haunting, where witnesses, apart from the unhappy Prichard family of 30, East Drive, included the local vicar, the Roman Catholic priest, the Mayor and the local Member of Parliament - to say nothing of reporters, "friends," and members of the Prichards' related families, this is unusual - if not (as it appears to be) unique in that the fragrance involved was produced in an apparently unprecedented fashion, and that, to "convince" sceptical Aunt Maude - who was, in fact, of an Evangelical disposition; a member of the Salvation Army - "Fred" the poltergeist put on what we may, without irreverence, call "a repeat performance by request." The phenomena involved are too numerous to be listed here, but it may be mentioned that the poltergeist's drumming in the Prichard house was clearly heard by the coal-miners on their way to work. "Fred" used to announce his presence - even when he cause a large grandfather's clock to hurtle down the stairs and smash to pieces in the hall - by "a delightful scent - a perfume like some heavily scented flower"; but when the Didymic Aunt Maude arrived, "Fred" introduced a subtle variety of performance: "a new and interesting ability which is found only in a rare minority of cases - 'interpenetration of matter.'"

One evening, as the Prichards were sitting in the lounge, an egg floated in through the door, poised itself very carefully in the air, then fell on the floor. As it exploded, the room filled with a delicious scent that Mrs. Prichard compared to a garden full of flowers. (Only Philip [Prichard, the 15-year-old son of the house] found it heavy and cloying.) When another egg floated into the room, [Mrs.] Jean Prichard rushed to the refrigerator, took out all the eggs, and put them into a wooden box. She then sat defiantly on the lid, convinced that, on this occasion at least, she'd got the better of the poltergeist. When another egg materialized in mid-air, and exploded like a scent-bomb, she jumped up and looked into the box. One egg was missing. She sat down on [the box] again; a moment later another egg exploded. It went on until all the eggs lay broken in the middle of the room, and the wooden box was empty. Yet Mrs. Prichard had sat firmly on its lid throughout. Mr. Nobody could dematerialize solid objects - or perhaps move them into another dimension and then back into our own.

"Fred," as Mr. Wilson observes, "seemed to take an unending delight in making messes"; but the Prichard house, after the explosion of all those ovoid scent-bombs, must have smelled like Floris's shop in Jermyn-street, St. James's. (We are not told what effect this demonstration of *apportage* had on Aunt Maude's incredulity - but she must have been severely shaken in her doubt...)

Far less pleasant an aroma literally stank out the house of Mrs. Harper, in Enfield, a suburb north of London, in a poltergeist haunting which began on 30th August, 1977, and which was fully (even though somewhat sarcastically) covered in a BBC radio-broadcast. The haunting was investigated by a Mr. Maurice Grosse, who also investigated the 1980 case of the "Croydon Poltergeist's," whose mischievous tricks were causing commercial chaos for the owners and manager of the King's Cellars. In the 1977 Enfield case, the Poltergeist's various disturbances were accompanied by "appalling stinks - like rotting cabbages," and when the medium, Gerry Sherrick, came on the scene, he, too, though in a trance-state, complained of the vegetable stink. And in 1980, in the underground bar of The King's Cellars, investigated by Mr. Colin Wilson as well as by Mr. Grosse, there was "a smell so disgusting - accompanied by the usual freezing cold - that they all felt sick." One may ask oneself at this point: was foetor rather than fragrance, in both cases, referable to the fact that each case was being studied by the same investigator, Mr. Maurice Grosse? Did his "subjective" hyperosmia communicate itself telepathically to others?

Since we still know so little about the causing of (must we call them?) hallucinations affecting one person and several persons, are the phenomena actually related? They are similar in that the olfactory nerves are involved in each phenomenon; but does this mean that the phenomena are actually related, or only seemingly so?

Much more study must go into the examination of each before we may confidently comment on the nature of either; but, at the beginning of every study, since we may not yet know what is relevant and what is not, every fact, no matter how apparently trivial, must be noted and recorded. Is it relevant that both Mrs. Harper and Mr. Grosse had a daughter, Janet, and that "Janet" is a name famous (or notorious, if you prefer) in the long record of British Witchcraft?

BOOK REVIEWS

The Metal Benders. By John Hasted. Routledge and Kegan Paul, London, 1981. ix + 279 pp. 9.75 pounds.

Reviewed by Harry Collins

I review this book on the assumption that I am not expected to have a strong opinion on the question of whether paranormal metal bending, or any other paranormal manifestation, is possible.

The book begins and ends with verses. It also contains an "apologia," a chapter on the history of the author's involvement with the phenomenon, a chapter on 'Metal Benders and World Reaction' and a chapter on "Some General Questions of Philosophical Interest." In between there are fourteen short chapters on the design of metal bending experiments, and on the results of such experiments; there is a chapter on "Some Psychological Effects"; there are six chapters on other paranormal phenomena such as poltergeists, levitation and teleportation, and there are two chapters on physics and the "many universes" interpretation of quantum theory which Hasted takes as a possible explanation for the effects observed.

The most important thing about the fourteen chapters on metal bending experiments is Hasted's central method and results. Instead of concentrating on gross visible paranormal deformations of metal in his experiments, Hasted looks for very small strains which do not necessarily result in any permanent deformation. He does this by attaching sensitive strain guages to the specimens. Then he asks subjects to try to deform the specimens without touching. The idea is that very small psychic effects may be more readily reproducible than large ones. On the face of it, Hasted has had considerable success with this method, and he is able to present a portfolio of successful experimental results of increasing elaboration. These chapters (or perhaps some sub-set of them) are the only things in the book that could have a positive effect on scientific opinion regarding the existence of paranormal metal bending. Were Hasted writing about some dull uncontroversial field of science his results would, no doubt, be taken at face value. But, Hasted, of course, is swimming against the tide, so his efforts will convince few. I suspect that Hasted's experimental design will be taken up and used by those few paraphysicists currently working on paranormal metal bending, and his results will give them encouragement. The design will at least enable them to get on with some experimental work in the absence of "star" subjects.

Now let us turn to the question of whether Hasted's work is likely to convince anyone else. The answer to this is almost certainly "no." The reason I can express such a degree of certainty is the context within which the fourteen chapters are set. The verses are forgiveable, the philosophical and psychological speculations are an indulgence that will irritate at worst, but the poltergeists, levitation and teleportation are a disaster. For example, in chapter eighteen Hasted describes certain disturbing experiences that took place in his house during and following a visit by Geller. The disturbances went on for several weeks and included the following incident which I present verbatim from pages 170-172:

"On 23 December, despite the disturbances, preparations for Christmas were going ahead. We had ordered a turkey from our butcher and, in addition, a second one which would be purchased from us and taken away by our friend David Jenkins. David was

living on his own and was faced with the prospect of cooking Christmas dinner for his visiting relations. His local butcher was unsatisfactory, whereas we had every confidence that ours would offer a good bird.

"He arrived to collect his turkey during the evening, but it was past 11 o'clock when we all went into the kitchen to present it to him. It was wrapped in a plastic bag and was resting on a tray on the bare white plastic table-top. Beside the turkey, on the tray and wrapped in another plastic bag fastened with wires, were the giblets, lever, etc.

"Suddenly a brown object appeared on the table in front of us, and I thought for a moment that it might be a leaf that had floated in through a window. But it was in fact a turkey liver, and we checked that one was no longer in the sealed plastic bag with the giblets. It resembled the other turkey liver, which we found to be safely in its own bag in the larder.

"Lynn had at that moment told David that he could make the giblets into soup. But what appeared were not the giblets but only their near neighbour, the liver.

"There was no smear of blood on the white table, such as the liver would have made if it had moved along the surface. There had been no sound. And there seemed to us no normal explanation of how the event occurred. I did not keep the liver for pathological examination, but I did check with our butcher that it was actually a turkey liver.

"This event was one of the most significant I had observed, since the liver in all reasonable certainty started from its situation inside the sealed plastic bag, and finished outside it. All three of us saw first of all an expanse of white table, and immediately afterwards a piece of liver on it. There were no holes in the plastic bag, although it was not vacuum-tight."

"Livertation in London poultrygeist case: professor talks turkey on teleportation"? The critics could not wish for anything more easy to poke fun at. And because incidents such as this one are reported alongside the dry metal bending reports, to take a serious interest in the latter is to find yourself an ally of the "wacky professor."

Let me put this in slightly more technical terms. Hasted has broken the norms of scientific publishing. To have your findings believed, a certain style of presentation is necessary. The reporting must be distanced, it should be written in the third person passive tense. It should be technical and impersonal. It should be technical and impersonal. It should make it seem as though the experimenter played no greater part than the emotionless midwife to the birth of mankind's understanding of nature's timeless laws. In mixing up personal anecdote with the more sober general reporting of the metal bending chapters, Hasted has spoilt his case. Secondly, where bizarre and heterodox results are to be reported, it is sensible to deliver the minimum of sensation in each dose. The conservative

scientific public may be willing to try to digest a little bit of the unusual, but to serve up a great multicoloured goblet is asking too much.

I believe Hasted has presented his work in this way because he is a naively honest man who thinks that scientists are all truthful and interested in the truth. He hasn't noticed that credibility is not the same as truthfulness, or if he has noticed it, he has decided to ignore it. Personally, I have no opinion about the turkey liver, though I have never witnessed any related event myself. Personally I am glad there are people around who value truth above credibility, but the scientific community does not.

BOOKS BRIEFLY NOTED*

- * Listing here does not preclude later full review.
- * Critical annotations are by Marcello Truzzi.

Abell, George O., and Barry Singer, eds., Science and the Paranormal: Probing the Existence of the Supernatural. New York: Charles Scribner's Sons 1981. 414+xi pp. \$17.95. An important but highly uneven anthology with strong original contributions by the editors and others but with poor reprinted papers including an unfortunate reprinting of Carl Sagan's much criticized paper on Velikovsky (which might have been excusable if the editors' had at least acknowledged the critical reactions against it). A central problem permeating the volume is displayed in the title: confusion of interest in the paranormal, which is a naturalistic term, with interest in the supernatural. The editors mix occultism with protoscience and pseudoscience and include even exobiology; thus, the book lacks an integrative analytic basis. Nonetheless, there are many excellent papers, and the editors have tried to be constructive in their skepticism; so, this is probably the best single general volume by critics of the paranormal in its many manifestations despite its pretense at speaking for science and rationalism when representing only the dominant viewpoint within current science. Certainly a book that should be read by all proponents of the paranormal while critics should recognize its flaws. Highly recommended.

Alcock, James E., Parapsychology: Science or Magic? A Psychological Perspective. New York: Pergamon, 1981. 224+ xi pp. \$35.00 hardbound, \$17.50 paperback. Perhaps the most important critical work on parapsychology published in the last ten years. The first section of the work dealing with magic versus science is weakest and neglects a vast literature, but many chapters are brilliant and Alcock has done his homework and presents a first-rate analysis that should be read carefully by all proponents of psi. A major weakness may be Alcock's failure to fully recognize that many of the cognitive errors he so well describes may also be made by critics as well as proponents of psi. Highly recommended.

Baran, Michael, Atlantis Reconsidered: A New Look at the Ancient Deluge Legends and an Analysis of Mysterious Modern Phenomena. Smithtown, N.Y.: Exposition Press, 1981. 85pp. \$6.00. A highly speculative attempt to integrate a great deal of esoteric material resulting a "solution" to UFOs as piloted by Atlanteans using the secrets of magnetic force. In the genre of Berlitz and Bergier with additions of Donnelly and Cayce. The scientist-author understandably uses a pseudonym.

Burnham, Kenneth E., God Comes to America: Father Divine and the Peace Mission Movement. New York: Lambeth Press, 1979. 167pp. \$14.95. A major sociological study of the Father Divine movement which emerged from the author's ethnographic doctoral dissertation. Particularly of interest for discussion of the socio-cultural context of the movement and its survival since Father Divine's death. A welcome study.

Cohen, Daniel, The Great Airship Mystery: A UFO of the 1890s. New York: Dodd Mead, 1981. 212 xii pp. \$9.95. An excellent survey of the granddaddy UFO flap by a sympathetic skeptic. Fascinating and well researched. Recommended.

Di Stasi, Lawrence, Mal Occhio (Evil Eye): The Underside of Vision. Berkeley, Cal.: North Point Press, 1981. 160pp. \$12.50. An extended philosophic essay starting with folklore of the evil eye and the "pervasive anxiety associated with vision" to the mother goddess and consciousness. Handsomely printed and provocatively presented.

Douglas, Graham, Physics, Astrology and Semiotics. London: G.J. Douglas (7 Graham Road; Stockwell, London SW9), 1982. 71pp. 90 pence, paperback. An unusual attempt to integrate structural features of astrology, thermodynamics, and digital communication in an attempt to create a "rapprochement between astrology and social sciences." A very creative effort which struck me as more reasonable than I at first expected. I don't presume to evaluate the effort, but did find it interesting and was impressed by the scope and references cited.

Dretske, Fred I., Knowledge and the Flow of Information. Cambridge, Mass.: MIT Press, 1981. 273+xiii pp. \$18.50. An important materialistic work presenting an information theory interpretation of knowledge and cognitive processes. Significant integration of epistemological and cognitive theory in an interdisciplinary and unified perspective.

Dundes, Alan, ed., The Evil Eye: A Folklore Casebook. New York: Garland, 1981. 280pp. \$30.00. An excellent anthology constituting the best single work for those interested in the folklore of the evil eye. Dundes has done a splendid job drawing excellent pieces from a vast literature on the subject. I personally found the very amusing essay by Arnold van Gennep a marvelous opening essay. A pity that the market for such books is so small as to make them so expensive, for this is a fascinating collection that many ZS readers would enjoy. Recommended.

Ebon, Martin, ed., Miracles. New York: Signet/New American Library, 1981. 200pp. \$2.50 paperback. A wide-ranging collection of 19 articles discussing alleged miracles, most newly published in this anthology. The articles on non-Christian miracle claims (from Islam to Cheyenne Indians) are especially interesting, and "New Miracles Do Not Interest Us" by Dominican priest Reginald Omez (here reprinted) may surprise many non-Catholics. Not a scholarly collection but fascinating and well-selected pieces that constitute a good introduction to such lore.

Eigen, Manfred, and Ruthild Winkler, Laws of the Game: How the Principles of Nature Govern Chance. New York: Alfred A. Knopf, 1981. 350+xv pp. \$19.95. A remarkable book dealing with the universality of play in everything from molecular biology, physics, linguistics and aesthetics to Beethoven and Samuel Beckett. Probability theory and nature presented for pleasure and insight for gamester and anomalist alike.

Eisenbud, Jule, Paranormal Foreknowledge: Problems and Perplexities. New York: Human Sciences Press, 1982. 312pp. \$24.95. A broad-ranging look at psi in general from the perspective of a psychoanalytically oriented clinician. A literate and informed discussion, with interesting case examples, but not intended for the skeptical reader or aimed at convincing so much as exploring a presumed reality.

Ellenberger, Henri E., The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry. New York: Basic Books, 1981. 932+xvi pp. \$15.95 paperback. A most welcome paperback edition of the 1970 work which I simply can not recommend too highly. Ellenberger's scholarship is outstanding and revises much historical work done earlier, and the book should be read by anyone seriously interested in the history of psychiatry. ZS readers should find the early chapters dealing with hypnosis and magical healing of special interest. Highly recommended.

Fleck, Ludwig (ed. by T.J. Trenn and R.K. Merton and trans. by F. Bradley and T.J. Trenn), Genesis and Development of a Scientific Fact. Chicago: University of Chicago Press, 1981. \$6.95 paperback. A most important book in the history and sociology of science, largely neglected since its original publication in German in 1935. As the title suggests, the study deals with the social negotiation involved in scientific "discovery," in this case via a masterful study of the "invention" of the "fact" of syphilis. Highly recommended.

Gadd, Laurence D., and the Editors of the World Almanac, The Second Book of the Strange. Buffalo, N.Y.: Prometheus Press, 1981. 341pp. \$16.95. A supplement to the earlier World Almanac Book of the Strange, updating some areas and presenting new topics. Generally well-balanced but about half the size of the first volume and not a comparable bargain.

Gardner, Martin, Science; Good, Bad, and Bogus. Buffalo, N.Y.: Prometheus, 1981. 408+xvii pp. \$18.95. A most welcome collection of Gardner's many essays published over the years plus special postscripts for each essay bringing the reader up-to-date and describing in many cases the reactions of readers to his original piece. Like the little girl with the curl, when Gardner is good, he is very, very good (there may be no critic better), but when he is bad he is horrid. Gardner makes his views clear in his introduction. He feels that some claims are so extreme as to warrant horselaughs rather than serious argumentation. The problem is that many readers would not agree on which claims are thus beyond the pale of rational discourse. Gardner is an unblushing advocate against what he views as outlandish claims, and his background of knowledge in science, philosophy, and conjuring stand him in good stead. His erudition is vast, and one must respect him even when upset by his sometimes pseudo-critical stances. For example, if one reads him carefully, his description of a crank does not fit some of his prime candidates (e.g., Velikovsky clearly fails Gardner's major criteria set forth yet is seen by Gardner as a model crank). Gardner uses many tricks of the good lawyer arguing a case, and we jurors may not always agree with him, but he is a fine advocate and always worth reading if only for his excellence as a science writer. But it is imperative that the reader remember that Gardner is a self-confessed advocate and not--as I am afraid many scientists see him--as the gate-keeper to truth in science. A critic as capable as Gardner helps keep all of us interested in the paranormal more honest. We need more watchdogs like Gardner looking over our shoulders. But Gardner should be read

with the same concern for rigor, evidence and sound argument in his work that he so often finds absent in the works he attacks. This collection is, in effect, an up-date of Fads and Fallacies in the Name of Science, and it contains the same strengths and weaknesses. There is much dogmatism in Gardner's writings, but there is also erudition and brilliance. He makes no pretense at being a professional scientist, mathematician, or philosopher of science; but he is a first class popularizer of science, and we should be grateful for that. Highly recommended.

Gibbs, Jack P., Norms, Deviance and Social Control: Conceptual Matters. New York: Elsevier, 1981. 190+xii pp. \$25.00. A very important work on deviance, much of which is applicable to the sociology of deviance in science and the sociology of deviant belief systems.

Goodman, Felicitas D., The Exorcism of Anneliese Michel. Garden City, N.Y.: Doubleday, 1981. 257+xxii pp. \$12.95. An anthropologist and expert on possession cases examines a well-publicized 1976 German episode of exorcism which led to the subject's death and the conviction of the two exorcists of negligent homicide. Recommended.

Goran, Morris, Can Science Be Saved? Palo Alto, Cal.: R&E Research Associates, (936 Industrial Ave; 94303), 1981. 88+vi pp. \$9.95 paperback (+ \$1 shipping directly from publisher). A good hard look at the contemporary problems, especially funding, of the scientific community which includes discussion of anti-science opposition and misbehavior by scientists.

Grover, Sonja C., Toward a Psychology of the Scientist: Implications of Psychological Research for Contemporary Philosophy of Science. Washington, D.C.: University Press of America, 1981. 92+x pp. \$7.50 paperback. A good brief survey of the case for greater consideration of psychological (subjective) factors in the scientific process. A book that should be read carefully by critics of anomalies. Verges on psychologism but provocative and a useful introduction to a growing literature.

Gutting, Gary, ed., Paradigms & Revolutions: Applications and Appraisals of Thomas Kuhn's Philosophy of Science. Notre Dame, Ind.: University of Notre Dame Press, 1980. 340+viii pp. \$18.95 hardbound, \$7.95 paperback. An excellent anthology of major papers critical and supportive of Kuhn's work as it effects philosophy, social science, the humanities, and the history of science.

Hogarth, Robert, Judgement and Choice. New York: John Wiley and Sons, 1980. 250+xi pp. \$24.25. A general work on the psychology of decision, intended to help people make better decisions. Of special interest for those of us concerned with evaluating extraordinary events.

Holroyd, Stuart, Alien Intelligence. New York: Everest House, 1979. 231pp. \$9.95. A look at all sorts of alien intelligence claims, from extra-terrestrial to primates to cybernetics and psychics. Not really a scientific work, but covers a lot of territory in very entertaining fashion.

Hoyle, Charles A., Witchcraft. Carbondale and Edwardsville: Southern Illinois University Press, 1981. 166+x pp. \$19.95 hardbound, \$10.95 paperback. An excellent short survey of the various approaches to the subject of witchcraft: the orthodox, skeptical, anthropological, psychological,

pharmacological, transcendental, and occult. Perhaps the best introductory book to the subject. Recommended,

Hurwood, Bernhardt J., Vampires. New York: Quick Fox, 1981. 179pp. \$7.95 paperback. A general compilation of photographs, lore, interviews, and trivia about vampires. Some scholarly material, but basically a book for fun and Dracula movie buffs. Some excellent popular culture materials on vampirism unavailable elsewhere.

Johnson, Charles W., Jr., Fasting, Longevity, and Immortality. Haddam, Conn.: Survival, 1978. 213pp. \$3.00 paperback. A general review of the literature on fasting from the serious scientific to the esoteric and exotic. Particularly interested in the possible relationship between fasting and psychic energy re the paranormal.

Khalsa, Parmatma Singh, ed., A Pilgrim's Guide to Planet Earth: A Traveller's Handbook & New Age Directory. San Rafael, Cal.: Spiritual Community Publications, 1981. 320pp. \$8.95 paperback. A newly revised edition with an introduction by Edgar D. Mitchell, this is an international directory to "sacred places," esoteric and spiritual centers, bookstores, and eateries.

Knorr, Karin D., Roger Krohn, and Richard Whitley, eds., The Social Process of Scientific Investigation. Boston: D. Reidel, 1980. 320+xxx pp. \$15.95 paperback. An important collection of empirical studies of scientific practice growing from the radical tradition which sees science as derivative from larger social and political institutions. Essential reading for those concerned with the influence of social context and negotiation on what becomes termed scientific knowledge. Highly recommended.

Leith, Harry, compiler, The Contrasts and Similarities among Science, Pseudoscience, the Occult, and Religion. Toronto: York University, Dept. of Natural Sciences, Atkinson College, 1982, 3rd edition. 105pp. \$2.95 paperback. A highly useful bibliography of books and articles (from 194 journals) on everything from astrology to Vel'ovsky's theories. Apparently put together for student use and geared in part toward the York University library, the articles, alas, are listed by titles and without authors' names (though books' authors are listed). Occasional typographical errors in citations which may mislead the unwarned (e.g., on page 35, "Freud in Parapsychology" should be "Fraud in Parapsychology"). Highly recommended.

LeMaitre, T.R., Stones from the Stars: The Unsolved Mysteries of Meteorites. Englewood Cliffs, N.J.: Prentice-Hall, 1980. 185pp. \$9.95. A very speculative but fascinating work dealing with the anomalies found in meteor behavior. Given that meteorites are sometimes used to explain other anomalies (e.g., UFO reports), this book suggests they themselves represent an important mystery for science.

Linden, Eugene, Apes, Men & Language (Updated with a New Afterword). New York: Penguin, 1981. 328+xv pp. \$4.95. A popular survey of the work being done (and hotly debated) about the use of language by apes. The author strongly sides with the proponents of language learning by apes but oversimplifies the controversy considerably.

Loftus, Elizabeth, Memory: Surprising New Insights Into How We Remember and Why We Forget. Reading, Mass.: Addison-Wesley, 1980. 207+xv pp. \$5.95, paperback. A popularization of modern work on memory emphasizing its fallible character and the mechanisms which interfere to the point that Loftus finds it almost miraculous that we correctly remember anything. Those working with witnesses to anomalous events would be well advised to read this book.

Machlup, Fritz, Knowledge: Its Creation, Distribution and Economic Significance, Vol. I: Knowledge and Knowledge Production. Princeton, N.J.: Princeton University Press, 1980. 272+xxix pp. \$17.50. An important work giving the conceptual foundation for the volumes which will follow. ZS readers should find the section on "Qualities of Knowledge," especially the chapter on "Notions of Negative Knowledge" of special interest.

Markle, Gerald E., and James C. Petersen, eds., Politics, Science, and Cancer: The Laetrile Phenomenon. Boulder, Colorado: Westview Press, 1980. 190+xv pp. \$20.00 hardbound, \$9.50 paperback. The papers from the symposium at the 1979 meetings of the American Association for the Advancement of Science. Less concerned with the technical medical issues than the social-political character of this remarkably effective challenge to medical expertise and authority. Highly informative and generally well balanced. Recommended.

Mitchell, Janet Lee, Out-of-Body Experiences: A Handbook. Jefferson, N.C.: McFarland, 1981. 128+xii pp. \$13.95. A generally well-researched work on OBE's but accepts much controversial work unconvincing to those critical of the evidence. Nonetheless, many people who experience OBE's, and are unlikely to be convinced that they are illusions, should find this a welcome book in dealing with their anxieties about OBEs.

Morison, Robert Kingsley, An Experiment with Space. London: Ascent Publication, 1980. 64pp. \$9.50. An attempt to "initiate levitational research based on the vortex preinciple" towards the development of a "vortex levitational vehicle" capable of the sorts of actions described of allegedly alien space vehicles. Metaphysics and particle physics in an esoteric blend. A Foreword by the Earl of Clancarty. For those who seek to "master gravity."

Musick, Ruth Ann, The Telltale Lilac Bush & Other West Virginia Ghost Tales. Lexington: University Press of Kentucky, 1974. 189+xvii pp. \$6.50 paperback. A highly entertaining folklore collection of 100 fine tales.

Musick, Ruth Ann, Coffin Hollow and Other Ghost Tales. Lexington: University Press of Kentucky, 1977. 194+xix pp. \$14.00 hardbound, \$6.50 paperback. Excellent folk tales authoritatively presented. 96 tales from West Virginia.

Persinger, Michael A., The Paranormal, Part I. New York: MSS Information Corp., 1974. 248pp. \$7.25. A remarkable behavioristic analysis of the verbal behavior about psi experiences which deserves serious attention but has been almost completely ignored since its publication. A very creative approach to spontaneous phenomena with most interesting results. It is unfortunate that most psychical researchers have a strong bias against behavioristic approaches, for this volume introduces what may turn into a most fruitful approach to the anomalies studied. Recommended.

Persinger, Michael A., The Paranormal, Part II: Mechanisms and Models.

New York: MSS Information Corp., 1974. 195pp. \$6.00 paperback. An important extension of the earlier volume but of independent value especially for its discussion of possible physical explanations including ELF fields, infrasonic stimuli, high voltage static fields, and environmental Peltier effect. Recommended.

Rogo, D. Scott, Miracles: A Parascientific Inquiry into Wondrous Phenomena.

New York: Dial Press, 1982. 333pp. \$17.95. A very interesting book but essentially a parascientific rather than truly scientific inquiry in that the author clearly accepts the paranormal explanation while inclined to reject the purely supernatural one. This is clearly reflected in the absence of harsher criticisms cited (e.g., the secular humanist criticisms of the shroud of Turin are ignored as are critics like D.H. Rawcliffe) while concentrating on parapsychological and sometimes unreliable journalistic sources. Nonetheless, Rogo has dug deeply into his subject and this is one of his best works. Though I personally would like to see a more cynical approach to these claims, Rogo has tried to be both careful and fair to the proponents, and the book is well worth reading.

Roll, William G., Theory and Experiment in Psychical Research. New York:

Arno Press, 1975. 510+xiii pp. \$35.00. The first book publication of Roll's 1959 Oxford thesis (B. Litt.), with a new forward by the author, as part of the Perspectives in Psychical Research series edited by Robert L. Morris. An excellent study whose inclusion in this series is most welcome.

Samples, Bob, Mind of Our Mother: Toward Holonomy and Planetary Consciousness.

Reading, Mass.: Addison-Wesley, 1981. 205+xviii pp. \$6.95 paperback.

A philosophical plea for universal unity: holism all the way. What I was able to read didn't do a whole lot for me, but I liked the book's pictures.

Wade, Nicholas, The Nobel Duel: Two Scientists' 21 Year Race to Win the World's Most Coveted Research Prize. Garden City, N.Y.: Doubleday/Anchor Press, 1981. 321+xi pp. \$15.95. A fascinating history showing the human side of science and discovery and the significance of priority in science.

Watson, Peter, Twins: An Uncanny Relationship? New York: Viking Press, 1981. 207pp. \$12.95. An excellent popular survey on the recent research on separated twins done at the University of Minnesota which has revealed apparent startling similarities between twins. Watson discusses possible paranormal connections in terms of statistical probabilities and concludes against paranormal conjectures in reasonable fashion. Clear, entertaining, and instructive. Recommended.

Wegner, Willy, Dansk UFO-Litteratur 1971-1979: En Bibliografi. Skeptica (Postboks 8026; DK-9220 Aalborg Øst; Denmark), 1981. 295+xvi pp. No price indicated. An international bibliography on UFOs with 1585 references, annotated in Danish.

Werner, Elizabeth, ed., 1981-1982 International Guide to Psi Periodicals and Organizations (8th edition). Burbank, Cal.: Inner Space Interpreters Services (P.O. Box 1133; Burbank, CA 91507). 68pp. \$4.00 paperback. An extremely useful guide though with significant omissions (e.g., The Skeptical Inquirer). There is also a Supplement with additional listings available for 50¢. Recommended.